DOCUMENT RESUME

ED 462 532 UD 034 848

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TITLE School Choice in New York City after Three Years: An Evaluation of the School Choice Scholarships Program. Final Report.

INSTITUTION Harvard Univ., Cambridge, MA. Kennedy School of Government.; Harvard Univ., Cambridge, MA. Dept. of Government.; Mathematica Policy Research, Washington, DC.

SPONS AGENCY Spencer Foundation, Chicago, IL.; Smith Richardson Foundation, Inc., Greensboro, NC.; Donner (William H.) Foundation.; John M. Olin Foundation, Inc., Alton, IL.; David and Lucile Packard Foundation, Los Altos, CA.; Lynde and Harry Bradley Foundation, Milwaukee, WI.

REPORT NO PEPG/02-01

PUB DATE 2002-02-19

NOTE 130p.; Prepared as part of the Program on Education Policy and Governance, a joint program of the Taubman Center for State and Local Government and the Center for American Political Studies, Harvard University. For the Interim Report after Two Years, see ED 446 193. Also supported by the Achelis, Bodman and Milton and Rose D. Friedman Foundations.

AVAILABLE FROM For full text: http://www.ksg.harvard.edu/pepg/pdf/nyc%20yr3%20MPR PEPG%20full.rep%202.19.02.pdf.

PUB TYPE Reports - Research (143)

EDRS PRICE MF01/PC06 Plus Postage.

DESCRIPTORS *Academic Achievement; Black Students; Educational Environment; *Educational Vouchers; Elementary Education; Hispanic American Students; Low Income Groups; Parent Attitudes; Private Schools; Program Effectiveness; Racial Differences; *Scholarships; *School Choice; Tables (Data); Urban Schools

IDENTIFIERS African Americans; Latinos; New York (New York)

ABSTRACT

This report presents third-year findings from an evaluation of the School Choice Scholarships Foundation Program. In 1997, this program provided scholarships via a lottery to low-income, New York City children in grades 1 through 4 that allowed them to transfer to private schools. The evaluation compared scholarship to control students, using test score, survey, and school-level data collected in the year 2000. On standardized tests, students offered scholarships performed similarly to control students. Those who ever attended private schools did not outperform those who never attended private schools. Patterns of impact for Hispanic students differed markedly from patterns for African American students in regard to test scores. Parents reported that schools and classes attended by scholarship students were smaller than those attended by public school students. Private schools were more orderly than public schools. Parents of children who switched to private schools were much more satisfied with their schools than parents of public school students. Among students offered scholarships, 53 percent used them to attend private schools for 3 full years. Parents who

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declined scholarships generally did so because they could not afford the added tuition and expenses. Five appendixes include research data and findings. (Contains 26 tables and 38 references.) (SM)

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Education Policy and Governance

ED 462 532

School Choice in New York City After Three Years: An Evaluation of the School Choice Scholarships Program

Final Report

Daniel P. Mayer, Paul E. Peterson, David E. Myers,
Christina Clark Tuttle and William G. Howell

PEPG/02-01

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School Choice in New York City After Three Years: An Evaluation of the School Choice Scholarships Program

Final Report

February 19, 2002

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This report is submitted by Mathematica Policy Research, Inc., and the Program on Education Policy and Governance, Harvard University. Dr. Myers, the project director, and Dr. Mayer and Ms. Tuttle are employed by Mathematica Policy Research. Dr. Peterson is the director of the Program on Education Policy and Governance at Harvard, and Dr. Howell is an assistant professor in the Department of Political Science at the University of Wisconsin, Madison.

ACKNOWLEDGMENTS

This study was made possible with support from the David and Lucile Packard Foundation, the Spencer Foundation, the Smith Richardson Foundation, the Achelis Foundation, the Bodman Foundation, the Lynde and Harry Bradley Foundation, the Donner Foundation, the Milton and Rose D. Friedman Foundation, and the John M. Olin Foundation. We also could not have conducted the study without the cooperation of the School Choice Scholarships Foundation and the principals and staff from schools where the testing sessions were held.

In addition, we would like to thank the many people whose efforts have made this report possible—those involved in the design and implementation of random assignment, in the collection of data, and in the preparation of this report. Charles Metcalf and Donald Rubin provided insightful comments and assistance in designing the study. John Homrighausen and Kathy Sonnenfeld were adept and successful at designing and supervising the data collection and processing effort. Heather Hesketh provided expert programming support. Mark Dynarski provided valuable comments on our analytic approach. Finally, we are indebted to Carol Soble, Daryl Hall, Micki Morris, Sharon Clark, and Margaret Hallisey for helping to produce this report.

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EXECUTIVE SUMMARY

In 1997, the School Choice Scholarships Foundation (SCSF) announced that it would provide 1,300 scholarships so that children of low-income families in grades K–4 in the New York City public schools could transfer to private schools. Each scholarship, or "voucher," was worth up to $1,400 annually and could be used for up to four years at a religious or a secular school. The SCSF received applications from more than 20,000 students from February through April 1997. From the pool of applicants, scholarship recipients were selected in a lottery held in May 1997.

This report presents the third-year findings from an evaluation of the SCSF program in which students were randomly assigned to a treatment group (scholarship group) or a control group. The evaluation findings are particularly relevant to the current national debate about the impacts of vouchers on students and parents—especially in that the SCSF program is one of the largest of the current voucher programs in terms of enrollment and has yielded results for a racially and ethnically diverse population of low-income students. Similar randomized field trials of school voucher programs have been conducted in Dayton, Ohio, and in Washington, D.C. This summary highlights the key evaluation findings and briefly describes the study.

KEY FINDINGS ON OUTCOMES

Impacts on Test Scores After Three Years

- On standardized tests, students *offered* a scholarship generally performed at about the same level as students in the control group. More specifically, we used the Iowa Test of Basic Skills to assess students' performance in reading and mathematics and found that, overall, students in both groups performed about the same. Moreover, those who *ever attended* a private school did not perform at higher levels than those who *never attended* a private school. Nor did those who attended a private school for *three full years* perform at higher levels than those who did never attended a private school.

- The pattern of impacts for Latino students, however, differs markedly from the pattern for African American students. We found no impact of a scholarship offer or of attending a private school on the test scores of Latino students, but we found a significant impact on the test scores of African American students. After three years the composite test scores (a combination of math and reading) of African American students who were offered a scholarship were about 5.5 percentile points higher than the composite test scores of African Americans not offered a scholarship. The composite test scores of African American students who ever attended a private school (for one, two, or three years) was 7.6 points higher than the composite test scores of students who had never attended a private school. The composite test scores of African American students who attended a private school for three full years was 9.2 percentile points higher than the scores of students who had never attended a private school. Impacts of a voucher offer do not vary significantly by grade level.

- After the first year of the program, the overall impact of a voucher offer on composite test scores for African American students was 4.4 percentile points; after two years, the impact was 3.2 percentile points; and after three years, it was 5.5 percentile points. Changes in the impact of actually attending a private school were larger, starting at 5.7 percentile points in year one, falling to 4.4 points in year two, and then rising to 9.2 points in year three. The differences between years one and two and years one and three were not statistically significant, but the difference between years two and three was.

- There was no change in the impact of being offered a voucher or attending a private school on Latino test scores over time.

Impacts on School Facilities, School Climate, Parents' Satisfaction with Schools, and Parental Communication and Involvement

- As reported by parents, the schools attended by the scholarship students were smaller than the schools attended by public school students (382 students versus 519 students in each type of school, respectively). Class size was smaller as well—there were two fewer students in the private-school classrooms than in the public-school classrooms (26 students versus 28 students). Private schools were less likely than public schools to have a cafeteria, a nurse's office, or special programs for non-English speakers and students with learning problems. On the other hand, private schools were more likely to have computer laboratories, after-school programs, and tutors for individual students. No differences were found in music or art programs, or in programs for advanced learners. Nor were there differences found in the availability of child counselors, a gymnasium, or a library.

- Private schools were more orderly than public schools, according to parents. Compared with public school parents, private school parents were less likely to report that the following were serious problems at their child's school: students destroying property, tardiness, missing classes, fighting, cheating, and racial conflict. For example, 64 percent of the parents with a child in public school reported that fighting was a serious problem compared with 34 percent of the parents with a child in private school.

- Private-school students reported better learning conditions at their school than did public-school students. Sixty-five percent of private-school students said that students get along with teachers while only 49 percent of public-school students said the same. Private-school students were also more likely to report that students are proud to attend their school and that behavior rules are strict. They were also less likely to feel put down by teachers or to report a lot of cheating by other students.

- Students in private schools were asked to complete more homework than students in public schools. Sixty-four percent of the parents with a child in private school said that their child had more than an hour of homework per day, compared with 41 percent of the parents with a child in public school.

- Parents of students in private schools said that they received more communication from their school about their children than did parents in public schools.

- Parents with a child in private school will be less involved in their child's education than parents with a child in public school. For example, parents of public-school students reported that they helped their child with homework an average of 11 times a month, compared with 9 times a month for private-school parents.

- Compared with public-school students, private-school students were more likely to participate in church youth groups and attend religious services.

- Parents of children who switched from public to private schools were much more satisfied with their schools than parents of children who remained in the public schools. For example, when asked to grade their schools, nearly 42 percent of the parents with a child in private school gave their school an "A," compared with just 10 percent of parents with a child in public school.

KEY FINDINGS ON PARTICIPATION IN THE SCSF PROGRAM

- Among those offered a scholarship, 53 percent used it to attend a private school for three full years, 9 percent used it for the first two years but not the third, 12 percent used it only in the first year, 2 percent used it only in the second year, and 24 percent never used it.

- Parents who declined a scholarship most frequently gave the following reasons for doing so: they could not afford the added tuition and expenses not covered by the voucher (45 percent), they could not find a school in a convenient location (33 percent), and their child had special needs (14 percent).

- There are many similarities and some differences between the parents and students who used the scholarship for at least one year and those who did not. Baseline test scores were similar for scholarship takers and decliners; scholarship takers and decliners, and their parents were equally likely to have lived at their current residence for two years; and mothers of takers and decliners were equally likely to have been born in the United States. On the other hand, scholarship decliners were somewhat less likely than scholarship takers to have received special education services before the baseline testing session; mothers of scholarship takers were more likely to have attended college for some amount of time; and the average income of families of scholarship takers was $2,400 higher than that of scholarship decliners.

- Students who attended private school were no more likely than those who remained in public school to move from one school to another. Parent reports indicate that similar percentages of public and private school students remained in the same school throughout the school year. Similarly, the percentage of students who planned to attend the same school the next year was similar for the two groups. In contrast, public school students were more likely to "graduate" from one school level to the next, perhaps because private schools are more likely to have grades K–8 in the same school. Suspension rates for students in private school were less than those for students in public school.

THE EVALUATION

The evaluation of the SCSF program in New York City presented a unique opportunity to examine the impact of vouchers on students and parents for students switching to private schools. New York City has not only a racially and ethnically diverse population but also the largest school system in the nation. We computed the effects of vouchers on education outcomes by using a randomized experimental design, which allowed us to compare two statistically equivalent groups of students and thereby isolate the unique effect of vouchers on the outcomes of interests, including student test scores, school climate and facilities, and parents' involvement and communication with schools.

Mathematica Policy Research, Inc. (MPR) along with researchers at Harvard University and the Program on Education Policy and Governance at Harvard have joined together in conducting this evaluation, which includes data collection, analysis, and the reporting of annual findings. MPR has collected data four times on the same students and families since 1997 (1997, 1998, 1999, and 2000). For instance, The Iowa Test of Basic skills was given to students to measure their academic achievement in reading and mathematics. In addition, parents and students completed surveys so that we could learn more about students' educational experiences, parents' experiences with the schools, and their school-related plans for the upcoming year. The student response rate for each test administration was moderately high—100, 78, 65, and 67 percent in 1997, 1998, 1999, and 2000, respectively. The response rates were somewhat higher for the parent and student surveys than for the achievement tests.

I. INTRODUCTION

In this report, we present results from the third follow-up data collection for the evaluation of the School Choice Scholarships Foundation program. This privately funded voucher program, begun in 1996 by the School Choice Scholarships Foundation (SCSF), was designed to allow for the collection of high-quality information about student test-score outcomes and for parental and student assessments of public and private schools. Taking advantage of the fact that scholarships were awarded by lottery, the evaluation was designed as a randomized field trial.

The evaluation findings reported in this document are based on test-score data and survey data collected in spring 2000. The findings build on the data already collected at baseline and at each of the first two follow-up periods in 1998 and 1999. The spring 2000 findings are also augmented with school-level data for public schools in New York City. Mathematica Policy Research, Inc. (MPR) has joined with researchers at Harvard University's Program on Education Policy and Governance to conduct this evaluation, which includes data collection, analysis, and reporting of the evaluation findings.

Many researchers, interest groups, political leaders, and policy analysts have debated the desirability of continuing and expanding school choice programs (Brandl 1998; Coulson forthcoming; Cobb 1992; Bonsteel and Bonilla 1977; Peterson and Hassel 1998; Ascher, Fruchter, and Berne 1996; Carnegie Foundation for the Advancement of Teaching 1992; Gutmann 1987; Levin 1998; Fuller and Elmore with Orfield 1996; Rasell and Rothstein 1993; Cookson 1995). Unfortunately, high-quality information that might inform this debate is limited. Although many studies comparing public and private schools have been published, they have been criticized for comparing dissimilar populations. And despite statistical adjustments made to account for the dissimilarities, it remains unclear whether findings reflect actual differences between public and private schools or simply differences between both the students who attend

the schools and the families of the two student populations (Coleman, Hoffer, and Kilgore 1982; Chubb and Moe 1990; Neal 1996; Goldberger and Cain 1982; Wilms 1985).

The best way to make sure that two populations are similar on the broadest possible set of characteristics is to assign individuals randomly to treatment and control groups. Recently, a number of education studies have used random assignment studies to estimate the effects of various education interventions. For instance, the Tennessee STAR study, which randomly assigned students to small or large classes, found that smaller classes had positive effects on students in kindergarten and first grade (Mosteller 1995; Krueger 1999). Another example of the random assignment of students to treatment and control groups is the national evaluation of Upward Bound, funded by the U.S. Department of Education, which has examined program effects on a variety of outcomes, including credits earned in high school subjects (such as mathematics and science) and college enrollment (Myers and Schirm 1999).

Until recently, random assignment has not been used to study the question of school choice. After the SCSF evaluation was initiated, two other random assignment evaluations were started in Washington, D.C., and Dayton, Ohio (Howell et al. 2000).

In view of its following characteristics, the SCSF program provided an opportunity to conduct a rigorous random assignment study:

- A lottery randomly allocated scholarships to applicants.
- An independent evaluation team administered the lottery.
- Before the lottery, baseline data on student test performance and family background characteristics were collected from the students and their families.
- Follow-up survey and test data were collected annually for several years after students were offered scholarships.

In this report, we provide information about the SCSF program three years after students started using their school-choice scholarships. We describe the program sponsored by the SCSF; the data collection, analysis, and reporting procedures used by the evaluation team; and findings from the evaluation for the third year of the program.

II. PROGRAM DESCRIPTION

In February 1997, SCSF announced that it would provide 1,300 scholarships in the amount of $1,400 annually for at least three years to children from low-income families currently attending public schools. The scholarship could be applied toward the cost of attending a private school, either religious or secular. After announcing the program, SCSF received initial application forms from over 20,000 students between February and late April 1997.

To be eligible for a scholarship, children had to be entering grades one through five, living in New York City, attending a public school at the time of application, and be a member of a family with an income that qualified children for the federal School Lunch Program. To ascertain eligibility, students and an adult member of their family had to attend verification sessions to document family income and the child's public-school attendance.

Because many more families than originally projected applied for scholarships, MPR randomly selected families for scholarships through a two-stage procedure. First, as families applied for scholarships, they were invited to eligibility assessment and data-collection sessions. Initially, all families were invited to the eligibility assessment and data-collection sessions. However, after it became clear that more families would be attending the sessions than could be accommodated, we began randomly selecting applicants for the sessions. After completion of the first stage, families that attended the sessions and met the eligibility requirements were then randomly selected for the scholarship or control group. To ensure that all families from the different sessions had the same chance of being selected for the scholarship group, we adjusted

the second-stage selection probabilities to reflect the differential chances of being invited to the verification sessions.

The final lottery was held in mid-May 1997. MPR administered the lottery; SCSF announced the winners. Within the parameters established by SCSF, all applicants had an equal chance of winning the lottery. SCSF, in consultation with the evaluation team, decided in advance to allocate 85 percent of the scholarships to applicants from public schools whose average test scores were lower than the citywide median. Consequently, applicants from these schools, who represented about 70 percent of all applicants, were assigned a higher probability of winning a scholarship. In the information reported in the tables in this report, we have adjusted the results by weighting cases differentially so that they can be generalized to all eligible applicants who would have attended the verification sessions had they been invited regardless of whether they attended a low-performing school. After the lottery, SCSF assisted families in finding private-school placements.

III. DATA-COLLECTION PROCEDURES AND RESPONSE RATES

The evaluation procedures used for the third follow-up data collection were similar to those used during the baseline, first, and second follow-up data collections. Below, we describe the procedures and present response rates for each round of data collection.

A. Collection of Baseline Data

During the eligibility verification sessions, students were asked to take the Iowa Test of Basic Skills (ITBS) in reading and mathematics. Students in kindergarten applying for a scholarship for first grade were exempted from the test requirement. Parents were asked to fill out questionnaires that asked about their satisfaction with the school their child was currently attending, their involvement in their child's education, and their demographic characteristics.

The eligibility sessions took place during March, April, and early May 1997 on Saturday mornings and on vacation days. The sessions were held at private schools, where students could take tests in a classroom setting. In most cases, private-school teachers served as proctors under the overall supervision of the staff of MPR.

While the child was taking the test, the adult accompanying the child to the testing session responded to the questionnaire in a separate room. This procedure had the advantage of giving parents time to complete the questionnaire and the opportunity to ask any questions concerning the meaning of particular questions. Parents were informed that their responses would be held in strict confidence and used for statistical purposes only. Questionnaires were available in both English and Spanish.

Given the likelihood that a variety of caretakers might accompany children to the sessions, the questions were designed to allow any caretaker familiar with the child's school experiences to respond to the questionnaire. Although grandmothers and other relatives and guardians also filled out the questionnaire, a child's parent answered the questions in over 90 percent of the cases. For ease of presentation, we refer to opinions expressed as those of parents.

Because scholarships were allocated by a lottery, there were few differences between students offered and students not offered scholarships (see Appendix A). For example, there were no statistically significant differences between the two groups in the mother's level of education, family income, number of children living in the home, or overall reading or mathematics test scores of the children in the study.[1]

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[1] These findings differ slightly from those initially reported in Peterson et al. (1997). After the first follow-up data collection, we revised our weighting procedures to include post-stratification adjustments. While some of the baseline comparisons changed, the first follow-up impacts estimates remained fairly stable.

B. Collection of Third-Year Follow-Up Data

The lottery created two statistically equivalent groups of families: (1) a scholarship group with 1,000 families and (2) a control group with 960 families.[2] Families in both groups were invited to attend sessions in April, May, and June 2000 in which students again took the ITBS in reading and mathematics and parents completed surveys that asked a wide range of questions about the educational experiences of their oldest child within the age range eligible for a scholarship. Students were also asked to complete short questionnaires.

Testing and questionnaire administration procedures were similar to those followed during the baseline and the second follow-up sessions. Both the scholarship students and students in the control group were tested in locations other than the school they were currently attending.

Table 1 shows the response rates for the third follow-up survey and test, along with response rates for the baseline and second follow-up surveys and test administration (see tables section on page 41). We present results separately for the treatment and control groups and distinguish between students who did not attend testing sessions and those who completed too few items on the reading and mathematics tests to be scored by the test publisher, Riverside Publishing. Seventy-two percent of the families selected for the evaluation participated in the sessions held in spring 2000. This satisfactory response rate was achieved in part because SCSF conditioned the renewal of scholarships on participation in the evaluation; nonscholarship winners selected to become members of the control group were compensated for their expenses and told that they could automatically reapply for a new lottery if they participated in the follow-up sessions.

As shown in Table 1, the participation rate was similar for the treatment and control groups: 71 percent of the families offered scholarships participated in the evaluation compared with 72

[2] Procedures used to construct the two groups and to collect first-year and second-year follow-up information are described in Jennifer Hill, Donald B. Rubin, and Neal Thomas (1997).

percent of the families in the control group. Sixty-seven percent of the students participating in the evaluation attended the testing session, including 69 percent of those offered scholarships compared with 65 percent of students in the control group.

The percentage of completed tests is lower than the family survey response rate because some students did not complete enough questions to have their tests scored and some parents who did not attend the testing sessions completed the surveys over the telephone. Several reasons may account for incomplete tests. For example, a few students decided for one reason or another that they did not want to complete all the items. In a few cases, sessions concluded before students had time to complete a section of the test. Finally, some students had never taken a standardized before and may have found the experience overwhelming; these students were excused from the testing session.

Although the background characteristics of respondents and nonrespondents in the third-year follow-up, as measured in the baseline survey conducted in 1997, resembled one another in most respects, they differed significantly along some dimensions (see Appendix A). Among the treatment group, respondents were less likely to be white or black and more likely to be Hispanic other than Puerto Rican. Mothers of respondents were more likely than mothers of nonrespondents to have been born outside the United States, more likely to state their religious affiliation as Catholic, and less likely to be food stamp or welfare recipients. Respondents had average incomes at baseline of around $9,800 compared with $8,800 for nonrespondents. Respondents were also less likely to speak English at home.

Members of the control group who participated in the third-year follow-up were less likely than nonrespondents to be black and more likely to be Hispanic other than Puerto Rican. The mothers of respondents were more likely not to be working, and they originally reported an average income of around $9,000 compared with $10,000 for nonrespondents. Respondent

families were also more likely to be collecting social security and Medicaid and less likely to speak English at home. Finally, families were more likely to report Catholic religious affiliation and to note that their child had received help for a disability.

To adjust for nonresponse in the statistical analyses, we used an analytic model to predict nonresponse based on a variety of background characteristics. We then applied the predicted probability of not responding to adjust the sample weights. Because differences between respondents and non-respondents were small, the addition of these weights had only minor effects on estimates of impacts. Appendix B provides a more detailed discussion of the procedure.

IV. DATA ANALYSIS AND REPORTING

In third-year data were useful for answering three questions about the effects of school choice:

1. What was the impact of the offer of an SCSF scholarship on a group of low-income scholarship applicants as measured by test scores and as perceived by the applicants themselves?
2. What was the impact of ever attending a private school, whether for one, two or three years?
3. What was the impact of attending a private school for three full years? Are any impacts observed in the first and second years sustained? Do impacts increase with the number of years in the program? Or do impacts dissipate?

The first question asks: what happens when school vouchers are offered to families? What are the impacts on the population of low-income families interested in taking advantage of school choice? The first question is similar to a question often asked in medical research: What will happen if a particular pill is marketed? How will the health of potential users be altered whether or not all patients use the pill as prescribed?

The second and third questions seek to know what happens when children from low-income families switch from a public to a private school. The question provides information concerning the consequences of actually attending a private school, not just the effects of an offer to do so. In medical research, the parallel questions are: What are the consequences of taking a pill for one to three years? What are the consequences of taking a pill for a full three years?

In addressing all three questions, we took advantage of the fact that a lottery was used to award scholarships. That is, we were able to compare two groups of students who were similar, on average, in almost all respects except that the members of the control group were not offered a scholarship. As a result, differences in outcomes can be attributed to the effects of the scholarship offer and not to measured or unmeasured differences between students in the two groups.

The analytic techniques needed to answer each question differ in important ways. The first question can be answered straightforwardly by comparing the responses of those who were offered a scholarship with the responses of the control group. To compute the impact of a *scholarship offer* on children's test scores, we estimated statistical models that accounted for the following: whether a student was offered a scholarship, baseline reading and mathematics test scores, and variables that define the randomization process (that is, blocking or stratification variables). Baseline characteristics were included to adjust for chance differences between the characteristics of treatment- and control-group members and to increase the precision of the estimated impacts. We used a similar approach to compute the impacts of the program as measured by the parent and student survey responses. In analytic models predicting parent and

student responses, we did not, however, include baseline data other than a treatment indicator and variables used to define the randomization process.[3,4]

To compute the effects of *attending private school* on students' test scores, we estimated a statistical model that accounted for baseline test scores, variables used to define the randomization process, and whether students attended private or public school. The test score analyses provide estimates of the impact of ever switching to private school and the impact of attending private school for three years. The impacts of switching to private school were estimated by using an instrumental variable (IV) estimator; the instrument was the treatment indicator, which was determined by the random assignment process.

Use of the IV estimator to estimate the impact of ever attending a private school requires us to assume that the offer had no impact on the year-three test scores for students who were selected for the scholarship and never used it. In other words, we estimate impacts for all those who attended private school, whether for one, two or three years. If benefits increase with the amount of time a student attended private school, the estimate provides a lower-bound estimate

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[3] Randomization increases the chances that treatment and control groups will be similar and, when the samples are large, that impact estimates will therefore be unbiased. Although including baseline test-score data in the survey analysis could have increased precision, it would have resulted in loss of data because kindergarten students were not tested at baseline. The test-score results focus on students in grades 1–4 at baseline, while the results from the analyses of parent and student satisfaction generalize to all who applied and were eligible for a scholarship.

[4] Since all eligible children within a family could receive a scholarship, some families had two or more children in the evaluation. The presence of multiple children from the same family produces clustering effects. When clustering is present and analyses are conducted under the assumption of simple random sampling—that is, that all observations are independent—researchers may underestimate the standard error of the estimated impact, overestimate test statistics, and conclude inappropriately that a difference between the treatment group and the control group is statistically significant. To approximate the true standard error more precisely, we estimated the standard errors for the impact estimates by using the bootstrap method (Stine 1990; Fox and Long 1982). This method provides a direct estimate of the variability in the treatment impact without the need to make an assumption about the independence of the observations in the sample.

of the impact of switching to private school. Use of the IV estimator to estimate the impact of attending private school for three years requires us to make an alternative assumption. In this estimation, we assume the offer of a scholarship had no average effect, either positive or negative, on those who did not use the scholarship for the full three years—the 16 percent of the treatment group participating in the third year of the evaluation who did not ever use the scholarship or the 6 percent who used the scholarship for just one year, or the 9 percent who used it for two years. In other words, the differences in test scores of the treatment group, as compared to the control group, is assumed to be produced solely by those remaining in private school for three years—and that never attending a private school or switching back and forth between public and private schools had, on average, neither a positive or negative effect. If switching had positive effects on student test scores, these results over-estimate three-year impacts. If this kind of switching had negative effects on student test scores, these results under-estimate three-year impacts.

To compute program impacts on the parent and student outcomes measured with survey items, we used a similar approach; however, we did not include the baseline test scores to predict parent and student responses. Because parent and student surveys asked for information pertaining to the student's current school, the survey analyses focus on the impact of attending private school in year three.[5] Appendix C provides a detailed description of the model estimation procedures.

Most of the findings presented in this report are statistically similar to the findings presented in the first- and second-year reports. To avoid repeating this fact, we make a point of

[5] It is important to note the distinction between the survey analysis and test-score analysis. The survey analysis focuses on the impact of being in private school in year three while the test-score analyses examine the impact of being in private school for three years.

emphasizing when third-year results differ from first- and second-year results. If we do not point out differences in third-year results, then those results are statistically indistinguishable from those of earlier years. Appendix C describes the procedures used to compare the results from year one with the results from year two and from year one with year three.

V. A CONTEXT FOR INTERPRETING PROGRAM IMPACTS

A. Response Bias

Program impacts based on parental and student surveys should be interpreted in light of potential response bias. That is, parents and students may exaggerate their responses to some items, such as satisfaction with their schools, time spent on homework, or educational expectations. No special weight should be placed on the actual frequency with which any particular type of event is said to take place. For example, one should not take too seriously the claim by children in third through fifth grades that they spend, on average, approximately one hour and 20 minutes a day on their homework.

B. Generalization of Findings

The results of the SCSF program evaluation cannot be generalized to a large-scale voucher program that would involve all children in New York City or other central cities because only a small number of low-income students in New York City public schools were offered scholarships. In addition, these students constitute only a small proportion of the students attending New York City private schools. A much larger program serving students from a variety of income levels could conceivably have a very different impact. In a larger, better funded program, more students might remain in private schools, altering the impact of a voucher offer. And more students might take advantage of the opportunity, changing the composition of students who would be utilizing the opportunity afforded them.

Nevertheless, the results of the SCSF program evaluation may say something about the likely impact of a small-scale, publicly funded voucher program serving low-income families. Earlier research indicates that the family background characteristics of those eligible to receive an SCSF scholarship and actual scholarship applicants differ only modestly (Peterson et al. 1997).

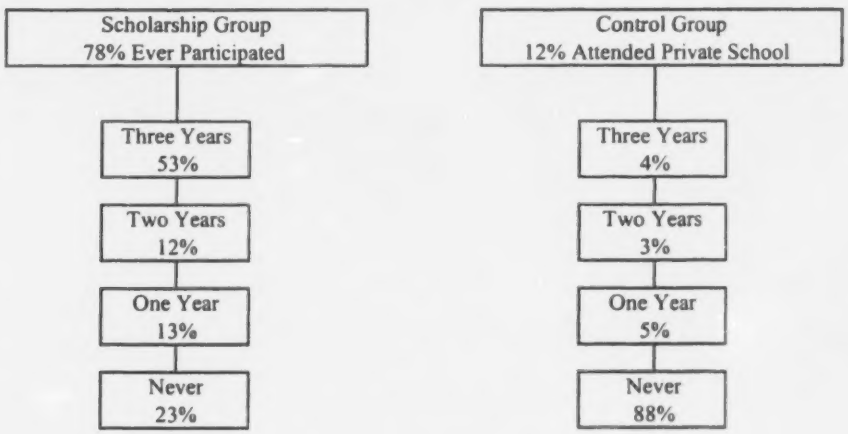
C. SCSF Program Participation and Members of the Control Group Attending Private Schools

Not all those offered a scholarship attended private school. Before the 1997–1998 school year, SCSF offered scholarships to 1,374 children. Administrative records show that, by the end of the third year, about 78 percent of these children had ever used a scholarship: 53 percent of the children had used a scholarship for three full years, 12 percent used one for two years, and 13 percent used one for only one year (see Figure 1). Most families who decided not to use a scholarship based their decision on financial reasons, recognizing that the $1,400 scholarship does not cover the full cost of tuition. (Later in this report, we provide a more detailed analysis of the reasons given by parents for their school selections and for leaving the program).

If all children randomly offered a scholarship had attended private school and none of those in the control group had done so, then the impact of the offer would be identical to the impact of attending private school. However, when not all families offered a scholarship make use of it, the estimated impact of the offer is reduced proportionately. Moreover, as shown in Figure 1, we found that about 12 percent of the control group attended private school for at least one year: 4 percent attended for three years, 3 percent attended for two years, and 5 percent attended for one

FIGURE 1

COMPLIER STATUS OF THE SCHOLARSHIP
AND CONTROL GROUPS



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year. Control group members attending private schools also reduced the estimated impact of private schools.[6]

If all children randomly assigned to the scholarship group had attended private school and all children in the control group had attended public school, the "treatment differential" would have been 100 percentage points. The private-school attendance patterns of the treatment and control groups suggest that the treatment differential is 66 percent for those who ever attended private school and 49 percent for those who attended private school for three full years.[7]

D. Participants Versus Nonparticipants

A frequently expressed concern about school vouchers is that only more advantaged families will take advantage of the voucher opportunity. To examine this question we examined those who used the scholarship for at least one year (ever takers) compared with those who never used the scholarship (never takers).

Table 2 shows no significant differences in the initial test scores between those who were offered and used a scholarship and those who did not use it. We present the scores in terms of national percentile rankings (NPR). NPR scores show a student's relative position or rank relative to other students who are in the same grade and who were tested at the same time of year. The initial reading scores collected during the 1997 baseline testing session averaged 23

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[6] Figure 1 uses administrative data provided by SCSF and pertains to *all* students in the study sample. Among those who participated in the third year evaluation 16 percent of the treatment group did not ever use the scholarship, 6 percent used the it for just one year, 9 percent used it for two years, and 68 percent used it for three years.

[7] We computed these treatment differentials (49 and 66 points) by using two approaches. First, we compared the percent of treatments with three years of exposure to private school (53 percent) with the percent of the control group that reported attending private school for three years (4 percent). Second, we compared the percent of the treatment group that attended private school for one or more years (78 percent) with the percent of the control group with the same pattern of private school attendance (12 percent).

NPRs for both the treatment and control groups. In mathematics, the difference—only one NPR point—was not a statistically significant. Those who took advantage of the scholarship were also just as likely as never takers to have lived at their current residence for two or more years.

There were, however, several differences between the two groups as related to economic well-being. For example, mothers of students ever using the scholarship were more likely to have more than a high school education. In particular, 54 percent of mothers of children using the scholarship had more than a high school education compared with 45 percent of mothers of children not using the scholarship. Economically, those who used the scholarship were in better circumstances than those who did not use it. The reported family income of scholarship users was about $2,400 higher than that of never takers. Mothers of ever takers were also more likely to be working full time, less likely to be on welfare, and less likely to be looking for work.

Racial/ethnic differences were also apparent. African Americans were more likely to have used the scholarship. Whites were less likely to use a scholarship. African Americans made up 49 percent of those who used the scholarship and 38 percent of those who did not. Whites represented 3 percent of those who used a scholarship and 14 percent of those who did not. There is no statistical difference between the percentage of Latinos who did or did not use the scholarship. However, 80 percent of those who used the scholarship reported that English was the main language spoken in their household compared with only 72 percent of those who did not use the scholarship.

Those who did not use the scholarship were also more likely than those who used it to report (at baseline) that their children were receiving special education services related to a disability or learning problem (15 versus 10 percent).

Those who declined the scholarship in year three were asked why they turned down the opportunity. As shown in Table 3, they cited three reasons most frequently: they could not

afford the tuition and expenses (45 percent), they could not find a school in a convenient location (33 percent), and their child had special needs (14 percent). About equal proportions of families gave the following reasons for declining the offer: the school of their choice did not have enough space for their child (7 percent), the quality of the private school was not acceptable (5 percent), they moved away from the area in which the private school attended by their child was located (5 percent), the child was suspended or expelled (5 percent), the child was not asked to return next year (5 percent), the private school wanted to hold the child back a grade (4 percent), and the child did not pass the private school's admission test (5 percent). Thirteen percent of the families reported "other" reasons for declining the offer.

The fact that cost was cited most frequently is not surprising given that the $1,400 voucher does not cover full tuition and expenses at private schools. The median tuition, according to parents of children attending private schools, was $2,000, and the median additional expenses for uniforms, school activities, books, supplies, and related items was $500. Therefore, most families who accepted an SCSF scholarship needed to find approximately $1,100 per child in supplemental funds.[8]

We explored the cost issue by inquiring of parents how they paid the tuition and additional expenses if the scholarship did not cover the full cost. We asked parents to list more than one source of revenue, if appropriate. Parents most frequently said that family income was the main source of funds (80 percent of scholarship users). Twenty-six percent said that relatives and

[8] Despite suspicions that families with more children would be less likely to be able to raise the supplemental funds and therefore would be more likely to decline the scholarship, a logit analysis revealed that no relationship exists between family size and the probability of declining a scholarship when other factors were taken into account.

friends helped out, and 5 percent said that their child had received a separate scholarship directly from the school.

E. Selecting a School

Critics and proponents of school choice often debate the importance of educational considerations in families' selection of a school. Critics argue that low-income families are more concerned about location, sports programs, or religious instruction than they are about academic quality per se. For example, the Carnegie Foundation for the Advancement of Teaching said that "when parents do select another school, academic concerns often are not central to the decision" (Carnegie Foundation for the Advancement of Teaching 1992). Similarly, an American Federation of Teachers report on the Cleveland voucher program said that parents sought scholarships not because of "'failing' public schools" but "for religious reasons or because they already had a child attending the same school" (Murphy, Nelson, and Rosenberg 1997). However, some researchers have found that low-income parents, like other parents, place the highest priority on the educational quality of the school (Schneider, Teske, Marschall 2000).

From a list of considerations, parents were asked to select the three most important considerations that they had in mind when selecting a school. Parents who accepted the offer of a scholarship most frequently mentioned academic quality (listed by nearly 63 percent of the parents; see Table 4); their other considerations included school discipline (46 percent of parents), religious instruction (34 percent), and teacher quality (32 percent). Almost 30 percent listed safety, and 22 percent listed what is taught in class. Almost 20 percent mentioned class size and a convenient location. Considerations mentioned by only a small fraction of the parents included school facilities, the sports program, and the school attended by the child's friends.

In sum, educational considerations seemed predominant, questions of social order (discipline and safety) and religious instruction followed, and the facilities and sports program were the least important.

F. Obtaining a Placement in the School of Choice

Compared with 61 percent of the control group, over 80 percent of parents who received a scholarship offer reported success in finding a school that met their needs (see Table 5). Parents were asked to identify why they thought that their choice went unfulfilled (parents could list more than one reason). The offer of a scholarship reduced from 31 to 11 percent the proportion of parents who said they could not afford their preferred school (see Table 5). Although parents most often mentioned the cost factor, families awarded a scholarship gave other reasons for not attending a school of their choice, including, in order of frequency, no reason given by the school (8 percent of parents), no space available (4 percent), and transportation problems (4 percent).

VI. THE IMPACT OF THE SCHOLARSHIP PROGRAM ON SCHOOL EXPERIENCES

One issue of considerable debate is the type of school experience a child will undergo as a result of a voucher intervention. Critics of choice say that public schools have better facilities and more elaborate programs capable of serving a diverse population and that choice will lead to ethnic and racial segregation (Murphy, Nelson, and Rosenberg 1997). Supporters of choice claim that private schools have the necessary facilities for and are more successful in integrating all children into a common framework. They also assert that private schools are, on average, more integrated than public schools (Greene 1998).

To address these issues, we asked parents about the facilities, programs, ethnic composition, and disciplinary climate in public and private schools. As noted, because the surveys asked for

information pertaining to the student's current school, the analysis focuses on the impact of attending private school in year three.

A. School Facilities

The facilities in central-city public schools are expected to be larger, more expensive, and more well equipped than the facilities in central-city private schools low income students attend. With a few exceptions, reports from applicant parents in New York City are consistent with the conventional wisdom.

As estimated by parents, the effect of choosing a private school reduced the average size of their child's school by 137 students, or over 25 percent—from an average of 519 students to 382 students (see Table 6). Private-school parents reported at the end of the third year that their children's classes were smaller. The effect of using a scholarship was to reduce the size of the child's class by two students.

Parents also reported that private schools were less likely to have a nurse's office, a cafeteria, and special programs for non–English speakers and students with learning problems. The greatest difference was in the availability of programs for non–English-speaking students. Forty-nine percent of private-school parents reported that such a program existed in their school compared with 89 percent of control-group parents. Not all differences were as large; for example, 68 percent of private-school families reported that their private school had a program for the learning-disabled compared with 82 percent of the parents in the control group (see Table 6).

In a few instances, parents who switched to private school reported that their private schools had more extensive facilities and programs. For example, they were somewhat more likely than parents in the control group to say that their school had a computer laboratory, individual tutors,

and an after-school program. For other facilities and programs, however, such as gymnasiums and arts or advanced learner programs, no differences between the two groups were evident.

In sum, compared with public schools, classes in private schools and the schools themselves are smaller, but public schools offer a wider range of facilities and programs. The larger, more complex facilities do not, however, seem to satisfy the control-group parents. On the contrary, only 9 percent of the parents in the control group were very satisfied with public school facilities, as compared to 29 percent of the parents whose children were attending a private school (see Table 6).

B. Ethnic Composition of School

Switching from public to private school in New York City placed minority students in classrooms with majority students, but it also placed more minority students in classrooms which were completely made-up of a single race/ethnic group. When asked about the percentage of minority students in their child's classroom, 40 percent of control- group parents replied that everyone in the classroom was of *minority* background (see Table 7). Only 30 percent of the private-school parents gave the same response. However, when parents were asked what portion of the student's class was of the *same race/ethnic background* as the child, those in private school were slightly more likely to say that everyone was of the same background—11 percent of private-school parents as compared with 6 percent of the control group. Because the first question discusses minority students, which could include students from several racial and ethnic backgrounds, while the second focuses on the specific racial and ethnic background of the classmates, the responses to these two questions are not as inconsistent as they might initially appear.

Moving to private school increased the number of friends of a different race reported by students. When asked to indicate the racial background of their four best friends, the average

increase in inter-racial friendships was no less than 0.5 students. Students in private school were also just as likely as the control group to welcome a family of a different race next door and to reject the notion that people of other races are bad.

C. Children with Special Needs

The debate over school choice has, among other issues, focused on special education. Critics of school choice say that private schools cannot or do not serve the needs of those with physical and mental disabilities (Murphy, Nelson, and Rosenberg 1997). Defenders of school choice often claim that many of those diagnosed as disabled can learn in regular classrooms and that special arrangements can be made for others.

To address this question, parents were asked to indicate the biggest obstacle keeping their child from performing better at their school. Among the items they were invited to consider was the "lack of facilities and programs needed to address their child's special needs." Parents with children in private school were much less likely than control-group parents to say that their school lacked such facilities and programs. Only 7 percent of parents with children in private school expressed this concern as compared with 17 percent of control-group parents (see Table 8). The difference was statistically significant.

To explore the question of special needs further, we asked parents about their child's special education needs and the availability of school programs to meet those needs. The number of learning-disabled and physically disabled students in the evaluation was small; therefore, the findings should be interpreted with caution.

Eleven percent of private-school parents indicated that their child had learning difficulties; another 4 percent said that their child had a physical disability (see Table 9). There was very little difference reported in how well the schools attended to learning disabilities (21 versus 26 percent) or physical disabilities (37 versus 39 percent).

D. School Climate

If parent reports are accurate, the scholarship program had a substantial impact on the daily life of students at school. Private-school parents were more likely to report that the following were *not* a serious problem at their school: destruction of property by students, tardiness, missed classes, fighting, cheating, and racial conflict. For example, 34 percent of the parents who switched their children to private schools compared with 64 percent of their counterparts in the control group thought that fighting was a serious problem at their child's school (see Table 10). In addition, 39 percent of the parents in the scholarship group perceived tardiness as a problem compared with 61 percent of the control-group parents. Twenty-nine percent of private-school parents but 48 percent of parents in the control group said that destruction of property was a serious problem at their child's school.

Student reports of the climate in their school and classroom are not as sharply differentiated as those of parents, but they are still consistent with parental assessments. Table 11 shows that students in private school were more likely than control-group students to report that students "get along with teachers" and less likely to say that "there is a lot of cheating in this school."

As reported in the first- and second-year evaluation reports, public and private schools seem to use different control mechanisms for maintaining discipline (Peterson, Myers, and Howell 1998; Myers et al. 2000). Private schools placed greater emphasis on dress and orderliness; public schools rely more on sign-in sheets and hall passes. Almost all private schools seem to require students to wear a school uniform. No less than 97 percent of the parents reported that their private school required uniforms as compared with 49 percent of control-group parents (see Table 10). Similarly, 95 percent of private-school parents reported that certain types of clothing are forbidden, but only 70 percent of control-group parents reported the same. On the other hand, parents reported that public schools more frequently use sign-in sheets for parents and

visitors and hall passes for students. Ninety-seven percent of the control group reported that parents must sign in when they come to school, as compared to 88 percent of private-school parents reported such a regulation. To leave their class, control-group students must obtain a hall pass, according to about 88 percent of control-group parents, as compared to 79 percent of private-school parents mentioned a similar requirement.

E. Homework

After three years, parents who switched their children to private school continue to say that their children do more homework. Sixty-three percent of parents with a child in private school reported that their child had more than one hour of homework a day, whereas only 37 percent of control-group parents reported a comparable volume of homework (see Table 12). Private-school parents were also less likely than control-group parents to say that homework was too easy (6 versus 23 percent).

Student assessments of their homework were not as sharply differentiated as those of parents, but the differences, although statistically insignificant, were in the same direction. Students attending private school estimated that they spent, on average, 50 minutes per typical night on homework as compared with 45 minutes reported by control-group members (see Table 12). In one respect, student reports concerning homework differ significantly between the first and the second and third years of the program. After one year, students new to private schools were more likely than control-group students to report difficulty in keeping up with their homework (Peterson, Myers, and Howell 1998). After two and three years, the difference was no longer apparent (see Table 12 and Myers et al. 2000). Students were adjusting to the homework expectations of their new school, or the school was adjusting to the new students and changing its expectations of the students.

F. School Communications with Parents

Compared with control-group parents, parents of students in private schools said that they received more communication from their school about their child. Although no significant differences in regular parent-teacher conferences were reported, the data in Table 13 indicate that a higher percentage of parents with a child in private school reported the following:

- Being more informed about the child's grades halfway through the grading period
- Being notified when their child is sent to the principal's office the first time for disruptive behavior
- Speaking to classes about their job
- Regular parent-teacher conferences
- Participating in instruction
- Receiving notes about their child from the teacher
- Receiving a newsletter about school events
- Frequency of parents' nights

VII. THE IMPACT OF THE SCHOLARSHIP PROGRAM ON PARENT AND STUDENT BEHAVIORS AND ATTITUDES

A. Religious Practices

Since 98 percent of the scholarship users attended parochial schools, it is possible that a switch from public to private schools affected students' religious practices. Students in private schools were more likely than the control group to report that they both attended religious services and participated in church groups. Table 14 shows that over 60 percent of the scholarship students reported regular attendance at religious services as compared with 37 percent of students in the control group. Almost half of the scholarship students said that they participated in church groups as compared with 28 percent of the students in the control group.

However, religious instruction outside the school was just as frequent for the control group as for the scholarship students.

B. Parental Involvement in a Child's Education

Supporters of school choice claim that when parents actively choose a school, the family becomes more engaged in the child's education and that, together, schools and parents create a more effective educational environment for their children (Brandl 1998). Critics of choice argue that any observed differences in parental engagement in private schools are attributable to parental characteristics that would predispose the parents to greater involvement.

The evidence after three years provides little indication that school choice increases family engagement in education. In fact, in contrast to the first two years of the program when no differences in parental involvement in the schools was apparent for scholarship students versus the control group, the findings in year three suggest that control-group parents are slightly more engaged in their child's education (see Table 15). Parents were asked how often they helped their child with homework, talked with their child about school, attended school activities, and worked on school projects. With the exception of working on school projects, parents who switched their children to private school were slightly less involved than their counterparts in the control group. Control-group parents spent two days more per month helping their children with homework, and one day more per month helping their children with reading and mathematics, talking to them about school, and attending a school activity with their child.

C. Student Adjustment to Choice Schools

Scholarship students appear to have adjusted well to their new schools. They reported the same number of friends at school as did control-group students (see Table 16). And they were much less likely than control-group students to say that they often "feel made fun of" by other

students. Only 21 percent of the scholarship students versus 35 percent of the control group reported being "made fun of."

D. Peer Influence, Suspension Rates, and Expectations

A student's close friends may influence his or her educational performance. Table 16 reveals that private-school students reported a higher number of friends who received good grades than did the students in the control group. Further, private-school students were less likely than the control group to have as many friends who used bad language. These results are similar to those observed in the second year. In other respects, however, no differences in friendship patterns were observed between scholarship students and the control group. The number of close friends who liked school, got in trouble with teachers, or smoked cigarettes did not differ significantly between the two groups. Nor did the two groups of students report different levels of drug and alcohol usage among their close friends.

Although year-two findings revealed no difference in suspension rates between the two groups, the year-three findings show that students who switched to private school were less likely than control-group students to be suspended. While 7 percent of control-group parents reported that their child was suspended during the year, only 2 percent of parents with a child in private school reported a suspension. The difference is statistically significant.

We found no indication that attending private school increases student aspirations. The percentage of students reporting that they intended to graduate from high school or to go on to post-secondary education after leaving high school was the same for the scholarship users and the control-group students.

E. Parent and Student Satisfaction

Most studies of voucher programs for low-income minority families have found that families taking advantage of a scholarship are much more satisfied with their child's schooling than are families whose child remains in public schools (Peterson 1998; Schneider et al. 1998). Our third-year results confirm these findings. As noted above when discussing how schools handle students with special needs, we asked parents to name the biggest obstacle that keeps their child from performing better in school (see Table 8). The private-school parents were much less likely than the control group parents to cite any obstacle. In fact, 75 percent of parents who switched to private schools named no obstacles while only 17 percent of control-group parents said claimed no obstacles. The latter were more likely to list the following as obstacles: teacher quality (24 versus 5 percent), lack of discipline (17 versus 1 percent), lack of facilities/programs to address their child's special needs (17 versus 7 percent), friends (10 versus 4 percent), and problems in the neighborhood (2 versus 0 percent).

When asked to assess their school overall, families give higher marks to private schools. Over 40 percent of the scholarship users gave their school an "A" compared with 10 percent of the control group (see Table 17).

We also examined parental satisfaction with specific aspects of school life. On every aspect about which parents were questioned, parents with a child in private school were substantially more satisfied than control-group parents. The percentage of parents "very satisfied" with a private school was significantly higher for all of the following: school location, school safety, teaching, parental involvement, class size, school facility, student respect for teachers, teacher communication with parents with respect to their child's progress, the extent to which a child can observe religious traditions, overall parental support for the school, discipline, clarity of school

goals, staff teamwork, teaching, academic quality, the sports program, and what is taught in school (see Table 17).

Differences in student reports of satisfaction were in the same direction but not as great as those reported by parents. Students in fourth through seventh grades were asked to give their school an overall grade. Students in private school were less likely than those in public school to give a "D" or an "F."

F. Hawthorne Effects

It may be hypothesized that the SCSF program, like other innovations, has a Hawthorne effect, namely, that innovation and change alone enhance the level of parental satisfaction. If so, then the scholarship program might be expected to have a weaker impact on parental satisfaction after three years than after one or two years. In fact, differences in the percentage of private-school parents and control-group parents who voiced satisfaction did change significantly from year one to year three on 8 of the 16 aspects of school life about which parents were asked. For example, the difference between the private-school parents' and the control-group parents' satisfaction with class size was 33 percentage points at the end of the first year and 22 points at the end of the third year—a statistically significant change. Other statistically significant changes in parent satisfaction from the first to the third year were related to school facilities, discipline, clarity of school goals, parental support for the school, what is taught in the school, teaching values, and teamwork among school staff. Despite the drop in satisfaction, parents of scholarship students remained much more satisfied than control-group parents with every dimension of school life they were asked about (see Table 17).

In addition, parents were asked to give an overall grade to their child's school. Based on their responses, we found no evidence that the program's impact had a statistically significant decline. The impact of private schools on the probability that a parent would give his or her

child's school an "A" declined from 39 percentile points in year one to 32 percentile points in year three. After three years, parents of scholarship users were still much more likely than control-group parents to give their child's school an "A."

In sum, some indication suggests that there may have been a slight Hawthorne effect. Overall, however, after three years, private-school parents remained more satisfied with their child's school than did public-school parents.

G. Suspension Rates and School Changes During the School Year

It is generally thought that students perform better if they can remain in the same school throughout the school year and from one year to the next. In the context of the SCSF evaluation, then, the question is: Does school choice destabilize a child's educational experience? In his evaluation of the Milwaukee school choice program, John Witte (1991) said that one of his concerns was the high rate of attrition from private schools. And a number of critics of choice have raised questions about the readiness of private schools to expel students who do not "fit in" (Murphy, Nelson, and Rosenberg 1997). Other studies, however, have found that students from low-income families are equally likely to remain in the same school both within the school year and from one year to the next regardless of whether the school is public or private (Greene et al. 1998). In general, the findings from the evaluation of the SCSF program confirm the conclusion that school choice does not disrupt the education of low-income students.

As noted, suspension rates in the third year were lower for the private-school group than for the control group. Seven percent of the parents in the control group reported that their child had been suspended while only 2 percent in the private-school group reported suspensions.

In all three years, a very high percentage of all students in the study remained in the same school for the entire year (see Table 18), and we found no difference between the two groups in

school mobility rates.[9] In short, school mobility was very low and virtually identical for both scholarship users and similar members of the control group.

H. Plans for Next Year

Private-school students were more likely than control-group students to plan to attend the same school next year. Almost 80 percent of the families of students attending private school compared with about 60 percent of control-group families said that they expect their child to be back at the same school (see Table 19). However, 16 percent of the control group compared with only 5 percent of private-school parents gave "graduating" as the reason for the change in schools. Apparently, many of the students in public schools "graduate" from elementary to middle school, actually moving from one location to another, whereas private schools do not necessarily recognize the transition as a formal graduation and do not require a move to a different location. With consideration of differences in the organization of the school system, there seems to be no significant difference in mobility rates from one year to the next for the two groups of students.

While private-school parents were less likely than control-group parents to cite graduation as a reason for changing schools, they were more likely to cite school cost as a reason for changing schools. Four percent of private-school parents reported expense as a factor, although none of the controls reported the same. There is no statistically significant difference in the

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[9] The percentages may underestimate the actual rate of school mobility for both scholarship students and those in the control group. The families that did not attend questionnaire administration sessions probably were more likely to have moved, making it more difficult for evaluation staff to locate them. If so, the children in those families that could not be located would be more likely to have changed schools. In this regard, it is important to note that the response rate was lower for the control group than for scholarship users, suggesting that actual differences in school mobility rates may be even higher than reported rates.

percentage of parents reporting that their children will attend a different school next year as a consequence of quality issues, the school's inconvenient location, or the family's relocation.

VIII. THE IMPACT OF THE SCHOLARSHIP PROGRAM ON TEST PERFORMANCE

This third-year evaluation of the SCSF program in New York City provided an opportunity to estimate longer-term impacts of school choice on student test scores. Earlier reports describe impacts on test scores after one year and two years of program participation (Peterson, Myers, and Howell 1998; Myers et al. 2000). This section addresses two general questions about the impact of educational vouchers:

- What is the impact of offering private-school vouchers on students' academic performance?
- What is the impact of private-school attendance on students' academic performance?

We report on the impacts of the award of a scholarship and the impacts of attending private school on student performance on the Iowa Test of Basic Skills in reading and mathematics. We report results for each test separately and for the two combined.[10] The impact of a scholarship offer is reported as the effect on student national percentile rankings (NPR), which may vary between 0 and 100. Nationally, the median NPR score on the Iowa Test of Basic Skills is 50. For all students, we report the average impact of the offer of a scholarship on the students' combined test-score performance as well as separate estimates on reading and mathematics

[10] We combined the test scores to form a composite by taking the average of the reading and mathematics scores for each student. Students who had a zero NPR score on either the reading or mathematics tests were retained in the analysis.

scores (see Tables 20 through 22).[11] The tables also show average impacts for African American and Latino students; the two groups comprised more than 90 percent of the sample, and there were not enough students in other ethnic groups to perform a separate analysis.[12] Besides reporting results for all students and for students by race/ethnic group, we report results separately for students in grades four, five, six, and seven in year three and for two groups of combined grade levels (grades four and five and grades six and seven).[13] As mentioned, we estimate the impact of offering a scholarship, the impact of ever switching to private school, and the impact of attending private schools for three years.

A. Impact of a Voucher Offer on Year-Three Test Scores

The impact of an offer is affected by two factors: the proportion of students who use the offer to attend private school and the size of the impact of attendance at a private school among those who make use of the offer. As a consequence, one should not interpret the results as showing the impact of attending private school; instead, the results show the average impact on test scores that we would expect if a policy with parameters and context similar to those found in the SCSF experiment were implemented.

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[11] Students who were in kindergarten at the time of the baseline data collection are not included in the test score analyses because we did not test such children before offering scholarships.

[12] About 43 percent of those students identified as Latino are Puerto Ricans, about 40 percent are from the Dominican Republic, and the remaining 17 percent are identified as other.

[13] When describing a grade for a student, we have classified students according to the grade they were expected to be in at the end of the third year of the voucher program. Most but not all students were in fact in the designated grade; some were held back a grade while others skipped a grade. To facilitate accurate comparison, all students were tested as if they were in the expected grade.

Results from the SCSF evaluation show that, after three years, the offer of a scholarship had no overall impact on student performance on tests (see Tables 20 through 22); that is, students offered scholarships had about the same test scores as students in the control group.[14] In separate analyses of African American and Latino students, however, we found that a scholarship offer had a statistically significant positive impact on test scores of the former but no significant impact on test scores of the latter.

The combined test scores of African American students who received a scholarship offer were 5.5 NPR points higher than the combined scores of those not offered a scholarship (effect size = 0.20 of a standard deviation). Scores on the reading test for those offered a scholarship were almost 4.0 percentile points higher, and in mathematics, scores were 7.0 points higher (effect sizes are 0.14 and 0.26 of a standard deviation, respectively). For the combined test scores, the difference between the scholarship group and the control group was statistically significant. Differences in reading and mathematics scores were also significant.

We also examined the impacts of a voucher offer for each of the four grades that participated in the evaluation. We estimated these impacts both for all students and for African American and Hispanic students separately. When estimating impacts by grade level for the two ethnic groups, the number of observations was fairly small, ranging between 194 and 188. When

[14] Comparison of baseline characteristics of students in the treatment group and the control group, after three years, reveals some differences (see Appendix A, Table A-1), often for family socioeconomic status. For example, for the overall sample of students, we found that the baseline reading test scores were higher for the treatment group than for the control group and that the treatment group was more likely to receive Medicaid payments. To check the robustness of our impact analyses in light of the potential differences between the treatment and control groups, we estimated analytic models with the composite test score as an outcome and with the standard set of covariates we use in all of the test-score analyses, which includes baseline test scores, and we added family income and mother's educational attainment as predictors. Impact estimates with and without the additional predictors are similar, and we arrive at the same conclusions.

test scores for different ethnic groups are viewed separately by grade level, they demand a measure of caution in interpreting the findings. Estimates for ethnic-group and grade-level analyses are based on a fairly small number of observations (ranging between 104 and 188 for the grade-level comparison with ethnic groups). With numbers this small, statistical estimates are susceptible to random fluctuations (Kane, Staiger, and Geppert 2002). In addition, impacts must be fairly large before they can be detected at levels of conventionally employed statistical significance. When discussing grade-specific impacts, we focus on the effect on composite (combined reading and mathematics) test scores because such scores are based on a larger number of test items, thereby reducing the amount of random fluctuation.

When we estimated the impact of a scholarship offer separately by grade and for the grade groupings for the overall sample of students, we found no impacts on the combined test scores. Nor did we find grade-specific impacts on the reading or mathematics scores.

Neither did impacts on African American test scores vary significantly by grade level. In examining the impact of a voucher offer on African American students by grade level, we observed no statistically significant difference in the impacts among grade levels (see Appendix D). The average impact on combined test scores for the younger students was 5.0 percentile points and 5.5 points for the older grades (see Table 23). Both impacts are statistically significant. Further separation of the grade levels into individual grades showed that the scholarship offer had statistically significant impacts on the combined test scores of students in grades six and seven (effect size = 0.16 and 0.20, respectively) and positive but statistically

insignificant impacts for the students in grades four and five.[15] Among Latinos, the scholarship offer had no statistically significant impacts on test scores by grade level.

B. Impact of Attending a Private School on Year-Three Test Scores

In contrast to considering the offer of a voucher that may have gone unused, our second research question focuses on the impact of attending private school. We present two separate estimates. First, we describe the impact of ever attending private school. Second, we discuss the impact of attending private school for three full years.

1. Impact of Ever Attending a Private School

To estimate the impact of ever switching to private school, we estimate impacts for all those who attended private school, whether for one, two, or three years. If benefits increase with the amount of time students were in a private school, this underestimates impacts of attending a private school for the full three-year period.

Among all students ever switching to a private school, which includes students who may have attended a private school for one, two, or three years, the switch had no significant effect on year-three test scores (see Tables 20 through 22). Nor did the switch have any significant effect on the test scores of Latino students.

For African Americans, however, the results were noticeably different, as they were for the scholarship offer. The impact of ever switching to private school on the combined test scores of African American students was 7.6 NPR points. This impact shows that if African American students who sought to leave the public schools in New York City ever switched to private school instead of a remaining in the public schools, we would expect, on average, a difference of

[15] When computing effect sizes for subgroups (African Americans and Latinos and by grade), we used the pooled standard deviation for the complete sample.

7.6 points between the private-school and public-school groups (effect size = 0.37 of a standard deviation). The reading test scores of those ever switching to private school were, on average, 5.5 NPR points higher, and mathematics scores were 9.7 points higher. The estimate of the impacts of attending private school on each of the test-score outcomes was statistically significant.

When the impact of ever attending private school on African American test scores was examined by grade level, we observed no statistically significant difference in the size of the impacts (see Appendix D). The impacts for grades four and five combined were 6.9 percentile points and 7.3 points for students in grades six and seven. Both estimates were statistically significant. Within individual grades, the impacts ranged from 3.6 to 9.7 percentile points. Only the impacts for African American students in grades six and seven were statistically significant when each grade was considered separately.

2. Impact of Attending a Private School for Three Years

The previous analysis of the impact of ever switching to private school estimates the average impact for students attending private school, whether for one, two, or three years. Ideally, we would like to assess the impact of a full, three-year period of private-school attendance relative to three years in public school; however, we can only approximate such an impact in this evaluation. To compute the impact of attending private school for three years, we had to assume that there was no impact of attending for one or two years on third-year test scores; that is, there was no benefit or harm in attending private school for one or two years and then returning to public school. The results of attending private school for three years are similar to the impacts of ever attending private school even though different assumptions were made (see Tables 20 through 22): (1) among all students, there were no impacts on test scores; (2) among African

Americans, substantial impacts were observed; and (3) among Latino students, there were no impacts.[16]

For African American students, there was an impact of 9.2 NPRs on the combined test scores (effect size = 0.45 of a standard deviation). For the reading test scores, the impact of staying in private school for three years was 6.7 points, and on the mathematics test, the impact was 11.8 points (effect size = 0.30 and 0.52 of a standard deviation, respectively).

When the impact of attending private school for three years on African American student test scores was examined by grade level, we observed no statistically significant differences in the impact between grade levels (see Appendix D). The impact for students in the younger grouping was 8.5 percentile points, and in the older grades the average impact was 9.1 points. Both impacts were statistically significant. When impacts were estimated separately by grades, they ranged from 4.4 to 11.7 percentile points. Only the impacts for grades six and seven were statistically significant.

C. Impacts of Vouchers over Three Years[17]

The ideal voucher experiment for elementary school students would show whether the impact of attending private schools increased, remained about the same, or declined as students moved from the lower to the upper elementary grades. In such an ideal experiment, all students

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[16] The assumption used when computing the impact of ever attending a private school was that the voucher offer had no impact on those who never attended a private school. The assumption underlying the impact of attending for three years was that the voucher offer had no impact on those who attended a private school for less than three years.

[17] Given that the offer of, or use of a voucher, does not have an impact on Latino test scores and does have an impact on test scores for African Americans, we conducted a descriptive analysis to explore two hypotheses about why the difference in impacts might exist: one focuses on differences in observed characteristics of the schools attended by African American students and Latino students, and a second examines whether differences exist for first-generation and later-generation Latinos. Appendix E presents the results from these analyses.

randomly assigned to the voucher group would attend private schools for the duration of the experiment, and those in the control group would attend only public schools. However, the ideal cannot be achieved since students and families move back and forth between private and public schools. To approximate the ideal experiment and assess whether there are changes in impacts, we focused on results that show the impacts after years one, two, and three for those who had complied with the experimental protocols at the three points in time.

As might be expected, given the lack of significant impacts for the sample of all students in each of the three years (see Tables 23 through 25), we did not observe a statistically significant change in impacts between years one and three or among the intervening years (see Table 26).[18] For African American students, for whom we did find significant average impacts on the combined test scores in each year, we did not observe a statistically significant change between years one and two or years one and three; however, we did observe a statistically significant change between years two and three (see Table 26).[19] When we examined changes in the three follow-up years of testing for African American students, we found no significant change in test score impacts between years one and two and a statistically significant change between years two and three (Table 26).[20] The estimated average impact of private-school attendance on the

[18] In Peterson, Myers, and Howell (1998), we reported small positive and statistically significant impacts by using the first year of follow-up scores. That analysis relied on one-tailed statistical tests while the current report and last year's report (see Myers et al. 2000) focused on two-tailed tests. The use of two-tailed tests in the first-year report would have resulted in statistically insignificant overall impacts. We have moved to two-tailed tests in recent years so that consumers of the findings can better assess whether there are no impacts, positive impacts, or negative impacts.

[19] Appendix C describes procedures for testing change in impacts across years.

[20] Although we have focused the discussion of changes in impacts over time on the impacts on the composite test scores, we note that for all students and for African American students we found a statistically significant change in impacts between year two and year three on the math test scores. For all students, the impact on the math scores was about 1.9 points in year one, -0.6

composite test scores of African American students was 5.7 percentile points after one year, 4.4 points after two years, and 9.2 points after three years. In none of the three years of testing were there statistically significant impacts for Latino students, and there were no changes in impacts.[21,22]

Several randomized studies of the impact of vouchers on families and students who apply for them have now been completed. Although these studies tell us much about the effects of vouchers on those who use them, we are left with many questions about the role of vouchers as a public policy. For example, what is the impact of vouchers on students who choose to remain in public schools? Will vouchers prompt schools to compete for students? And will such competition result in better schools and, in turn, overall higher academic performance for all students? Furthermore, the policy debate would benefit from more information about the impact of vouchers of varying amounts and about why test scores for some groups go up while the scores for others remain unchanged (for example, African Americans versus Latinos, respectively). The answers to these questions lie in the continued execution of high-quality randomized experiments.

(continued)
points in year two, and 2.6 points in year three. For African American students, the impact was 7.0 percentile points in year one, 4.1 points in year two, and 11.8 points in year three.

[21] Similar results were found for (1) the impact of a scholarship offer, (2) the impact of ever switching to private school, and (3) the impact of attending for three years.

[22] As a check for the robustness of the over-time comparisons, we estimated the year-one and year-two results by using both the third follow-up and second follow-up sample weights. A similar pattern in the year-one and year-two results were observed; that is, somewhat numerically larger impacts in year one than in year two.

TABLES

TABLE 1

RESPONSE RATES
(Percentages[a])

	Baseline			First Follow-Up			Second Follow-Up			Third Follow-Up		
	Attended session	Test scored		Attended session	Test scored		Attended session	Test scored		Attended session	Test scored	
		Reading	Mathematics		Reading	Mathematics		Reading	Mathematics		Reading	Mathematics
Student Test												
Overall	100	79	73	78	75	74	65	63	59	67	65	65
Scholarship group	100	79	73	86	78	78	69	67	62	69	66	67
Control group	100	79	73	75	72	69	60	59	55	65	63	62
Family Survey												
Overall	100	N/A	N/A	82	N/A	N/A	74	N/A	N/A	72	N/A	N/A
Scholarship group	100	N/A	N/A	84	N/A	N/A	75	N/A	N/A	71	N/A	N/A
Control group	100	N/A	N/A	80	N/A	N/A	72	N/A	N/A	72	N/A	N/A
Student Survey												
Overall	100	N/A	N/A	75	N/A	N/A	66	N/A	N/A	68	N/A	N/A
Scholarship group	100	N/A	N/A	76	N/A	N/A	69	N/A	N/A	69	N/A	N/A
Control group	100	N/A	N/A	74	N/A	N/A	62	N/A	N/A	66	N/A	N/A

COMPUTER SOURCE—MPR: Response_rates.do; sty40103.do.

[a] All percentages based on total sample of all families (students) who participated in the second stage of the lottery.

TABLE 2

DEMOGRAPHIC CHARACTERISTICS FOR SCHOLARSHIP TAKERS AND DECLINERS[a]
(Percentages)

	Ever Takers[a] (1)	Always Decliners[a] (2)	Difference (3)
Family income[***]			
Less than $5,000	27	38	-12[***]
$5,000-$10,999	37	39	-2
$11,000-$24,999	33	21	12[***]
$25,000-$39,999	3	2	1
$40,000 or more	0	0	0
Total	100	100	
Average family income	10,024	7,590	2,434[***]
Family receiving government assistance			
Welfare	53	69	-16[***]
Social Security	11	12	-1
Mother's employment status[***]			
Full time	24	15	9[***]
Part time	16	15	1
Looking for work	44	55	-10[**]
Not looking	14	15	-1
Don't know	1	1	1
Total	99	101	
Percent of mothers at current residence for two years or less	20	24	-5
Highest level of education completed by mother[***]			
Some high school	21	24	-3
High school graduate or GED certificate	24	29	-5
Some college	43	31	12[***]
Graduated from a four-year college	8	10	-2
More than a four-year college degree	3	4	-1
Don't know	1	3	-2
Total	100	101	
Mother's ethnicity[***]			
Black	49	38	11[**]
White	3	14	-6[***]
Puerto Rican	17	22	-6
Latino other than Puerto Rican	25	24	0
Other	5	2	0
Total	99	100	

TABLE 2 (*continued*)

	Ever Takers (1)	Always Decliners (2)	Difference (3)
Mother's religious affiliation[***]			
Baptist	21	20	1
Other Protestant	18	12	7*
Catholic	52	49	3
Other religion	5	13	-8***
No religion	4	7	-2
Total	100	99	
Percent of mothers U.S.-born	56	56	0
Percent of households with English as main language	80	72	7**
Percent of children receiving any special education services related to a disability or learning problem	10	15	-5*
Baseline test scores (in national percentile rankings)			
Reading	23	23	0
Mathematics	17	16	1
(N)	608-1027	158-293	

COMPUTER SOURCE—MPR: FAY40201.do.

*Statistically significant at .10.
**Statistically significant at .05.
***Statistically significant at .01.
***Significant at .01 using the chi-square. The chi-square test was used to test for differences in the distributions of categorical outcomes between takers and decliners.

[a]Takers are defined here as students in the treatment group who ever made use of the scholarship; decliners are students in the treatment group offered a scholarship but never using it.

TABLE 3

REASONS WHY STUDENTS DID NOT TAKE SCHOLARSHIPS

	Percentage
Why is this child not now using his or her scholarship?	
Could not afford the additional tuition and expenses	45
Could not find a convenient private school	33
Has special education needs	14
Other	13
Was not given space in private school	7
Quality of private school not acceptable	5
Moved away from private school	5
Was suspended or expelled from private school	5
Was asked not to return to private school	5
Private school wanted to hold child back a grade	4
Did not pass private-school admissions test	3

COMPUTER SOURCE—MPR: sty40111a.do.

TABLE 4

PERCENTAGE OF SCHOLARSHIP TAKERS WHO RESPONDED THAT THE FOLLOWING WAS ONE OF THE THREE MOST IMPORTANT CONDIDERATIONS IN CHOOSING A SCHOOL

Considerations	Percentage
Academic quality	63
Discipline	46
Religious instruction	34
Teacher quality	32
Safety	29
What is taught in class	22
Convenient location	19
Class size	17
The school was the only choice available	9
The child went to a neighborhood public school	5
School facilities	3
Child's friends	1
Sports program	0

COMPUTER SOURCE—MPRE: sty40111.do.

TABLE 5

PERCENTAGE OF FAMILIES WHOSE CHILD ATTENDED PREFERRED SCHOOL
(Percentages)

	Scholarship Offered	Control Group	Scholarship Offer Impact
Attended preferred school	81	61	20***
Reasons for not attending preferred school			
Family could not pay the cost	11	31	-20***
Child did not pass admissions test	1	1	0
Family not member of affiliated church	1	1	0
No more space at school	4	5	-1
Applied too late	2	2	0
Transportation problems	4	3	1
Moved away from school	1	1	0
School location inconvenient	1	2	-1
Communication problems	2	1	1**
School not in zone or district	2	8	-6***
School did not offer special education	2	0	2***
No reason given by school	8	5	3
Other	1	1	0
On average, how long does it take this child to get from home to school each morning?[a]	15	16	0
(N)	1,392		

COMPUTER SOURCE—MPR: fay40100.do, fay40102- fay40105.do.

[a]The coding for the length of time it takes to get to school is as follows: 5 if under 10 minutes, 15 if 11–20 minutes, 25 if 21–30 minutes, 38 if 31–45 minutes, 53 if 46-60 minutes, and 61 if more than one hour.

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 6

SIZE AND QUALITY OF SCHOOL FACILITIES
(Percentages)

	Scholarship Offered[a] (1)	Control Group[b] (2)	Scholarship Offer Impact[c] (3)	Scholarship User[d] (4)	Control-Group Complier[e] (5)	Switch to Private School[f] (6)
Average school size	409	493	-84***	382	519	-137***
Average class size	24	25	-1***	26	28	-2***
Percentage very satisfied with school facilities	24	12	12***	29	9	20***
Percentage with the following resources:						
Special programs for non-English speakers	55	77	-22***	49	89	-40***
Special programs for learning disabled	70	78	-8***	68	82	-14***
Nurse's office	83	94	-11***	79	97	-18***
Child counselor	78	81	-3	77	83	-6
Library	89	92	-3	87	91	-4
Cafeteria	91	96	-5***	88	96	-8***
Special programs for advanced learners	54	58	-4	51	58	-7
After-school program	91	86	-5***	93	84	9***
Gymnasium	91	91	0	90	89	1
Arts program	79	81	-2	82	85	-3
Computer laboratory	91	84	7***	93	81	12***
Music program	80	78	2	83	80	3
Individual tutors	57	44	13***	61	38	23***
(N)	919–1,379			919–1,379		

COMPUTER SOURCE—MPR: fay40102.do, fay40103d.do.

[a]Those who were offered a scholarship, whether or not they made use of it.
[b]Those who were not offered a scholarship.
[c]Estimated impact of being offered a scholarship.
[d]Those who were offered a scholarship and identified by SCSF staff as having used their scholarship to attend a private school.
[e]Those in the control group who would have used a scholarship had they been offered one as described in Appendix C.
[f]Estimated impact of participation in the program during at least the third year, using a two-stage least squares model, as described in Appendix C.

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 7

ETHNIC AND RACIAL ISOLATION IN CLASSROOM
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control Group-Complier (5)	Switch to Private School (6)
What percentage of students in child's class are minority?						
Less than one-quarter	12	12	0	11	11	0
One-quarter to one-half	13	10	3	15	10	5
One-half to three-quarters	11	13	-2	13	16	-3
Three-quarters but not everyone	30	25	5	30	22	8
Everyone	34	40	-6**	30	40	-10**
Total	100	100		99	99	
What portion of the student's class is of the same race/ethnic background as this child?						
Less than one-quarter	19	17	2	19	16	3
One-quarter to one-half	20	18	2	23	19	4
One-half to three-quarters	20	25	-5*	20	28	-8*
Three-quarters but not everyone	28	31	-3	28	33	-5
Everyone	12	9	3*	11	6	5*
Total	99	100		101	101	
(N)	1,353-1,369			1,353-1,369		
Student reports (percentages)						
Number of friends of a different race[a]	2.8	2.5	0.3**	2.8	2.3	0.5**
Would not like having a family of a different race move next door	10	12	-2	10	13	-3
In general, experiences with people of other races have been bad	6	9	-3*	4	8	-4
(N)	1,614-1,640			1,604-1,629		

COMPUTER SOURCE—MPR: fay40107.do, sty40112.do, sty40113.do.

See notes to Table 6.

[a]The index is scored 0 if the child reports no friends of a different race, 1.5 for one to two friends, 3.5 for three to four friends, 5.5 for five to six friends, and 7.5 for seven or more friends.

TABLE 8

OBSTACLES THAT KEEP THE CHILD FROM PERFORMING BETTER IN SCHOOL
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control Group-Complier (5)	Switch to Private School (6)
Parents report as biggest obstacle keeping child from performing better						
There are no obstacles	58	23	35***	75	17	58***
Teacher quality	8	19	-11***	5	24	-19***
Lack of discipline at the school	5	15	-10***	1	17	-16***
Friends	7	11	-4***	4	10	-6**
Problems at home	1	2	-1	0	2	-2
Problems in the neighborhood	1	2	-1**	0	2	-2**
Lack of motivation	11	13	-2	8	12	-4
Lack of facilities/programs needed to address child's special needs	10	16	-6***	7	17	-10***
(N)	1,355			1,355		

COMPUTER SOURCE —MPR: fay40103c.do.

See notes to Table 6.

TABLE 9

SPECIAL EDUCATION FACILITIES AND PROGRAMS
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Children with physical disabilities	3	0	3***	4	-1	5***
Children with learning disabilities	10	6	4**	11	4	7**
(N)	1,392-1,393			1,392-1,393		

	Scholarship Offered	Control Group	Difference	Scholarship User	Control Group in Public Schools	Difference
Percentage that believe school doing "very well"' at attending to these need						
Physical disabilities[a]	.31	.44	-.13	.37	.39	-.02
Learning disabilities[a]	.24	.24	.00	.21	.26	-.04
(N)	42-156			42-156		

COMPUTER SOURCE —MPR: fay40103c.do, fay40104.do, fay40109.do, fay40119.do.

See notes to Table 6.

[a]These figures are calculated as a percent of those parents with disabled or non—English-speaking children, not as a percent of the entire population. Because these groups were not created by using random assignment, we report differences in means and not impacts.

TABLE 10

PARENT PERCEPTIONS OF SCHOOL CLIMATE
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Parents report as serious problem						
Fighting	44	62	-18***	34	64	-30***
Tardiness	46	59	-13***	39	61	-22***
Students missing class	37	52	-15***	30	55	-25***
Students destroying property	31	42	-11***	29	48	-19***
Cheating	35	41	-6**	29	40	-11**
Racial conflict	31	36	-5*	27	36	-9*
Parents report on school rules						
School uniform	85	57	28***	97	49	48***
Certain forms of dress forbidden	87	72	15***	95	70	25***
Visitors must sign in at main office	90	95	-5***	88	97	-9***
Hall passes required to leave class	82	87	-5**	79	88	-9**
(N)	1,241-1,377			1,241-1,377		

COMPUTER SOURCE—MPR: fay40102.do, fay40103d.do.

See notes to Table 6.

TABLE 11

STUDENT PERCEPTIONS OF SCHOOL CLIMATE

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Student reports (percentages)						
Students are proud to attend this school	61	56	5*	64	56	8*
Behavior rules are strict	67	62	5*	69	60	9*
Students get along with teachers	60	51	9***	65	49	16***
Feel "put down" by teachers	17	21	-4**	15	22	-7*
Teachers ignore cheating	15	18	-3	13	17	-4
There is a lot of cheating in this school	25	30	-5**	18	28	-10**
(N)	1,618-1,746			1,607-1,735		

COMPUTER SOURCE—MPR: sty40112.do, sty40113.do, sty40114.do, sty40115.do.

See notes to Table 6.

TABLE 12

HOMEWORK
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Parent reports						
Child has more than one hour of homework	57	42	15***	63	37	26***
Homework too easy	8	18	-10***	6	23	-17***
(N)	1,361-1,395			1,361-1,395		
Student reports						
Trouble keeping up with homework	24	24	0	24	24	0
Time spent on homework on typical night (in minutes)	47	44	3	50	45	5
Teachers return homework always or most of time	52	55	-3	51	56	-5
(N)	1,736-1,779			1,726-1,751		

COMPUTER SOURCE—MPR: fay40103c.do, fay40103d.do, sty40113.do, sty40115.do.

See notes to Table 6.

TABLE 13

SCHOOL COMMUNICATION WITH PARENTS
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Parents regularly informed about student grades	91	83	8***	97	83	14***
Parents receive notes from teacher	90	80	10***	93	76	17***
Parents receive newsletter	82	72	10***	84	68	16***
Notified of disruptive behavior	91	81	10***	91	75	16***
Parents speak to classes about jobs	44	37	7**	38	26	12**
Parents participate in instruction	65	53	12***	67	47	20***
Parent night	92	87	5***	97	89	8***
Regular parent-teacher conferences	93	92	1	95	93	2
(N)	1,091-1,361			1,091-1,361		

COMPUTER SOURCE—MPR: fay40103c.do.

See notes to Table 6.

TABLE 14

RELIGIOUS PRACTICES
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Student reports						
Religious instruction outside school	20	19	1	21	19	2
Attend religious services	52	38	14***	62	37	25***
Participate in church group	44	34	10***	47	28	19***
(N)	1,732-1,747			1,721-1,736		

COMPUTER SOURCE —MPR: sty40112.do, sty40113.do.

See notes to Table 6.

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TABLE 15

PARENTAL INVOLVEMENT IN CHILD'S EDUCATION
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Average number of times per month parents did the following:[a]						
Helped child with homework	10	11	-1***	9	11	-2***
Helped child with reading, mathematics	8	9	-1**	8	9	-1**
Talked with child about school	13	14	-1**	13	14	-1**
Attend school activity with child	4	5	-1**	4	5	-1**
Worked on school projects	5	5	0	5	6	-1
(N)	1,377-1,391			1,377-1,391		

COMPUTER SOURCE —MPR: fay40107.do.

See notes to Table 6.

[a] The index is scored 0 if a parent never did the activity, 3 for 1–5 times, 8 for 6–10 times, 13 for 11–15 times, and 18 for 16 or more times in the past month.

TABLE 16

STUDENT FRIENDSHIPS, BEHAVIOR, AND EXPECTATIONS
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Student reports						
Number of close friends at school[a]	6	6	0	6	6	0
Percentage of students who feel "made fun of" by other students	29	38	-9***	21	35	-15***
Percentage of students who report that they will						
Graduate from high school	66	63	3	71	66	5
Go on for further education after they leave high school	73	71	2	71	68	3
Number of close friends who						
Get in trouble with teachers	2.0	2.1	-0.1	1.9	2.0	-0.1
Use bad language	1.8	2.2	-0.4**	1.7	2.3	-0.6**
Smoke cigarettes	0.2	0.2	-0.1	0.1	0.2	-0.1
Drink beer or alcohol	0.2	0.2	0.0	0.2	0.2	0.0
Use illegal drugs	0.2	0.2	-0.1	0.1	0.3	-0.1
Like school	4.0	4.0	0.0	3.9	3.8	0.1
Get good grades	4.5	4.3	0.2*	4.6	4.3	0.3*
Parent reports (percentage)						
Child suspended in past year	4	7	-3**	2	7	-5**
(N)	1,305-1,730			1,305-1,719		

COMPUTER SOURCE—MPR: fay40102.do, sty40112.do, sty40113.do.

See notes to Table 6.

[a]The index is scored 0 if the child reports no close friends at school, 1.5 for one to two friends, 3.5 for three to four friends, 5.5 for five to six friends, and 7.5 for seven or more friends. The same scoring is used for the number of friends who get in trouble at school, who smoke cigarettes, who drink beer or alcohol, and who use illegal drugs.

TABLE 17

PARENTAL AND STUDENT SATISFACTION WITH SCHOOL
(Percentage)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Parent reports (Percent very satisfied)						
Observe religious traditions	32	30	2***	41	6	35***
Class size	25	12	13***	30	8	22***
Discipline	38	17	21***	51	16	35***
Academic quality	37	17	20***	48	14	34***
Student respect for teachers	39	17	22***	50	12	38***
Parental support	26	15	11***	33	14	19***
Teaching values	34	17	17***	44	15	29***
What taught in school	35	17	18***	45	14	31***
School safety	40	21	19***	48	15	33***
Teaching	41	20	21***	52	16	36***
Teacher-parent communication	41	25	16***	51	24	27***
Clarity of school goals	30	16	14***	38	14	24***
Staff teamwork	25	13	12***	31	10	21***
Sports program	16	9	7***	20	8	12***
School facility	24	12	12***	29	9	20***
Parental involvement	28	17	11***	34	16	18***
Location	47	27	20***	55	21	34***
Gave school an "A"	32	13	19***	42	10	32***
(N)	1,366-1,398			1,366-1,398		
Student reports						
Gave school an "A"	43	39	4	44	36	8
Gave school "D", "F"	6	9	-3*	4	9	-5*
(N)	1,805			1,794		

COMPUTER SOURCE —MPR: fay40102.do, sty40112.do, sty40113.do.

See notes to Table 6.

TABLE 18

STUDENTS CHANGING SCHOOL DURING SCHOOL YEAR
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Attended same school for entire school year	95	93	2	100	97	3
Reasons why did not attend same school for entire year						
Moved away	1	2	-1	0	2	-2
Quality of school	0	0	0	-1	0	-1
School too expensive	1	1	0	1	0	1
Suspended/expelled	0	0	0	0	-1	1
Preferred public school	0	0	0	0	0	0
Inconvenient location	0	1	-1	0	1	-1
Preferred private school	0	-1	1	0	-1	1
(N)	1,400			1,400		

COMPUTER SOURCE —MPR: fay40103c.do.

See notes to Table 6.

TABLE 19

SCHOOL MATRICULATION PLANS FOR NEXT SCHOOL YEAR
(Percentages)

	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Switch to Private School (6)
Child will attend same school next year	87	76	11***	79	60	19***
Reasons why student will not attend same school next year						
Quality of school	7	8	-1	4	6	-2
Moving	7	5	2	6	2	4
Graduating	8	15	-7***	5	16	-11***
Preferred private school	3	1	2*	2	-1	3*
Inconvenient location	3	3	0	2	1	1
School too expensive	4	1	3***	4	0	4***
Children in same school	2	2	0	1	0	1
Asked not to return	0	0	0	0	1	-1
Preferred public school	0	0	0	0	0	0
(N)	1,400–1,401			1,400–1,401		

COMPUTER SOURCE —MPR: fay40103h.do.

See notes to Table 6.

TABLE 20

YEAR-THREE COMPOSITE TEST-SCORE IMPACTS
(Percentile)

	Scholarship Offered			Ever Switched			Stayed for 3 Years			
	Scholarship Offered (1)	Control Group (2)	Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Impact (6)	Scholarship User (7)	Control-Group Complier (8)	Impact (9)	(N) (10)
All students										
Overall—average	27.58	26.65	0.93	25.98	24.64	1.34	27.70	26.20	1.49	1,250
Grades 4+5	26.10	25.84	0.26	24.24	23.87	0.37	25.51	25.08	0.43	680
Grades 6+7	29.43	27.43	2.00	28.24	25.40	2.84	30.13	27.12	3.01	570
Grade 4	27.40	27.03	0.37	26.39	25.84	0.55	27.56	26.88	0.67	331
Grade 5	24.80	25.04	-0.23	22.11	22.44	-0.33	23.65	24.04	-0.38	349
Grade 6	29.00	27.33	1.67	30.46	28.10	2.36	31.58	29.06	2.52	322
Grade 7	29.98	27.33	2.65	25.40	21.61	3.79	28.25	24.37	3.88	248
African American students										
Overall—average	26.55	21.05	5.50***	25.37	17.82	7.55***	26.83	17.60	9.23***	519
Grades 4+5	25.07	20.07	5.00**	23.68	16.78	6.90**	25.29	16.84	8.45**	283
Grades 6+7	28.31	22.80	5.51***	27.44	20.12	7.32***	28.70	19.64	9.06***	236
Grade 4	27.31	22.87	4.44	26.85	20.71	6.15	28.12	20.37	7.75	127
Grade 5	22.97	20.23	2.74	20.79	17.19	3.60	22.66	18.25	4.41	156
Grade 6	27.79	22.99	4.80*	29.57	22.89	6.69*	29.88	21.66	8.22*	130
Grade 7	28.93	21.33	7.60**	25.07	15.33	9.74**	27.45	15.74	11.71**	106
Latino students										
Overall—average	27.62	28.56	-0.95	26.58	27.98	-1.40	27.84	29.34	-1.51	637
Grades 4+5	25.88	26.93	-1.05	23.95	25.49	-1.54	23.99	25.78	-1.79	347
Grades 6+7	29.84	30.03	-0.19	30.17	30.45	-0.28	31.99	32.27	-0.28	290
Grade 4	26.75	26.13	0.62	23.81	22.85	0.97	24.26	23.07	1.19	178
Grade 5	24.94	27.66	-2.72	24.09	28.00	-3.91	23.72	28.29	-4.57	169
Grade 6	29.30	28.55	0.75	31.34	30.29	1.06	31.74	30.70	1.04	170
Grade 7	30.62	29.41	1.21	28.15	26.24	1.91	32.40	30.72	1.68	120

COMPUTER SOURCE—MPR: sty40304.do, sty40306.d0, sty40316.do, sty40318.do, sty40501.do, sty40502.do (impacts); sty40317.do, sty40319.do, sty40511.do, sty40512.do (means).

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 21

YEAR-THREE READING TEST-SCORE IMPACTS
(Percentile)

	Scholarship Offered			Ever Switched			Stayed for 3 Years			
	Scholarship Offered (1)	Control Group (2)	Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Impact (6)	Scholarship User (7)	Control-Group Complier (8)	Impact (9)	(N) (10)
All students										
Overall--average	27.30	27.03	0.27	25.77	25.38	0.39	27.71	27.28	0.43	1,250
Grades 4+5	26.44	26.62	-0.18	24.61	24.86	-0.25	25.78	26.08	-0.30	680
Grades 6+7	28.38	27.14	1.24	27.27	25.52	1.75	29.86	28.00	1.86	570
Grade 4	27.97	28.53	-0.56	26.45	27.26	-0.82	27.61	28.62	-1.01	331
Grade 5	24.91	25.53	-0.61	22.79	23.66	-0.87	24.11	25.11	-1.00	349
Grade 6	28.48	28.83	-0.34	29.41	29.89	-0.48	31.34	31.86	-0.52	322
Grade 7	28.24	25.16	3.08	24.59	20.19	4.40	27.97	23.46	4.51	248
African American students										
Overall--average	26.73	22.76	3.97**	25.30	19.85	5.45**	27.28	20.62	6.66**	519
Grades 4+5	26.06	22.71	3.35	24.19	19.57	4.62	26.35	20.69	5.66	283
Grades 6+7	27.52	22.88	4.64**	26.66	20.49	6.17**	28.49	20.86	7.63**	236
Grade 4	28.18	24.85	3.34	27.51	22.89	4.62	28.92	23.09	5.83	127
Grade 5	24.07	23.92	0.15	21.23	21.03	0.20	23.98	23.74	0.24	156
Grade 6	28.33	23.71	4.62	30.38	23.95	6.43	31.34	23.45	7.90	130
Grade 7	26.56	20.28	6.27*	22.49	14.45	8.04*	25.38	15.72	9.66	106
Latino students										
Overall--average	26.93	28.78	-1.85	25.86	28.59	-2.73	27.28	30.22	-2.94	637
Grades 4+5	25.46	26.98	-1.52	23.46	25.68	-2.22	22.96	25.55	-2.59	347
Grades 6+7	28.81	29.48	-0.67	29.16	30.17	-1.01	31.93	32.92	-0.99	290
Grade 4	26.82	27.68	-0.86	22.84	24.19	-1.34	23.12	24.78	-1.66	178
Grade 5	23.98	26.19	-2.21	24.10	27.27	-3.17	22.80	26.50	-3.71	169
Grade 6	28.03	30.61	-2.58	29.05	32.69	-3.64	30.48	34.06	-3.58	170
Grade 7	29.94	24.19	5.75	29.41	20.36	9.05	34.36	26.38	7.98	120

COMPUTER SOURCE—MPR: sty40304.do, sty40306.d0, sty40316.do, sty40318.do, sty40501.do, sty40502.do (impacts); sty40317.do, sty40319.do, sty40511.do, sty40512.do (means).

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 22

YEAR-THREE MATHEMATICS TEST-SCORE IMPACTS
(Percentile)

	Scholarship Offered			Ever Switched			Stayed for 3 Years			
	Scholarship Offered (1)	Control Group (2)	Impact (3)	Scholarship User (4)	Control-Group Complier (5)	Impact (6)	Scholarship User (7)	Control-Group Complier (8)	Impact (9)	(N) (10)
All students										
Overall--average	27.86	26.27	1.59	26.20	23.90	2.30	27.68	25.13	2.56	1,250
Grades 4+5	25.76	25.07	0.69	23.86	22.87	0.99	25.25	24.10	1.15	680
Grades 6+7	30.48	27.71	2.77*	29.22	25.30	3.92*	30.40	26.23	4.17*	570
Grade 4	26.83	25.53	1.30	26.33	24.42	1.91	27.51	25.15	2.36	31
Grade 5	24.69	24.55	0.14	21.43	21.22	0.21	23.20	22.96	0.24	349
Grade 6	29.51	25.82	3.68	31.52	26.32	5.21	31.82	26.27	5.55	322
Grade 7	31.72	29.49	2.23	26.21	23.03	3.18	28.53	25.27	3.26	248
African American students										
Overall--average	26.38	19.34	7.03***	25.45	15.79	9.65***	26.38	14.58	11.80***	519
Grades 4+5	24.08	17.43	6.65***	23.17	14.00	9.17***	24.24	13.00	11.24***	283
Grades 6+7	29.10	22.72	6.38***	28.23	19.75	8.48***	28.91	18.43	10.48***	236
Grade 4	26.43	20.89	5.54	26.20	18.53	7.67	27.33	17.66	9.67	127
Grade 5	21.86	16.53	5.33**	20.35	13.36	6.99**	21.34	12.77	8.58*	156
Grade 6	27.26	22.27	4.99	28.77	21.83	6.94	28.41	19.88	8.53	130
Grade 7	31.30	22.37	8.93**	27.65	16.21	11.45**	29.52	15.76	13.75**	106
Latino students										
Overall--average	28.30	28.35	-0.05	27.31	27.38	-0.07	28.40	28.47	-0.08	637
Grades 4+5	26.30	26.88	-0.58	24.45	25.30	-0.85	25.02	25.98	-0.96	347
Grades 6+7	30.87	30.58	0.29	31.18	30.74	0.44	32.04	31.61	0.43	290
Grade 4	26.68	24.59	2.10	24.78	21.51	3.27	25.40	21.36	4.04	178
Grade 5	25.89	29.12	-3.23	24.09	28.73	-4.64	24.65	30.08	-5.43	169
Grade 6	30.57	26.50	4.08	33.63	27.88	5.75	32.99	27.33	5.66	170
Grade 7	31.29	34.62	-3.33	26.88	32.12	-5.24	30.43	35.05	-4.62	120

COMPUTER SOURCE—MPR: sty40304.do, sty40306.d0, sty40316.do, sty40318.do, sty40501.do, sty40502.do (impacts); sty40317.do, sty40319.do, sty40511.do, sty40512.do (means).

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 23

YEAR-ONE, -TWO, AND -THREE COMPOSITE TEST-SCORE IMPACTS
(Percentile)

		Scholarship Offered			Ever Switched			Years Attended			
		Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)	N (10)
All students											
Overall--average		1.20	0.46	0.93	1.62	0.62	1.34	1.62	0.65	1.49	1,199 –1455
Grade, year 1	*Grade, year 3*										
2+3	4+5	0.07	-0.69	0.26	0.03	-0.93	0.37	0.03	-0.98	0.43	648 –766
4+5	6+7	3.47**	1.51	2.00	4.69**	2.07	2.84	4.69**	2.14	3.01	551 –689
2	4	2.67	-0.90	0.37	3.83	-1.21	0.55	3.83	-1.28	0.67	307 –370
3	5	-2.52	-1.69	-0.23	-3.45	-2.30	-0.33	-3.45	-2.41	-0.38	341– 396
4	6	2.06	0.54	1.67	2.72	0.76	2.36	2.72	0.80	2.52	313 –395
5	7	4.71**	2.88	2.65	6.14**	3.83	3.79	6.14**	3.85	3.88	238 –294
African American students											
Overall--average		4.43***	3.27*	5.50***	5.73***	4.29*	7.55***	5.73***	4.41**	9.23***	497 –623
Grade, year 1	*Grade, year 3*										
2+3	4+5	3.41*	1.34	5.00**	4.41*	1.71	6.90**	4.41*	1.75	8.45**	271 –332
4+5	6+7	6.13***	4.83**	5.51***	7.88***	6.44**	7.32***	7.88***	6.65**	9.06***	226 –291
2	4	6.83**	1.85	4.44	9.10**	2.37	6.15	9.10**	2.33	7.75	118 –152
3	5	-1.84	-1.93	2.74	-2.44	-2.43	3.60	-2.44	-2.59	4.41	153– 180
4	6	3.86	0.93	4.80*	5.24	1.31	6.69*	5.24	1.28	8.22*	122 –167
5	7	8.98***	7.92***	7.60**	10.31***	9.00***	9.74**	10.31***	9.77***	11.71**	104 –124
Latino students											
Overall-average		-0.66	-0.60	-0.95	-1.01	-0.82	-1.40	-1.01	-0.88	-1.51	612– 709
Grade, year 1	*Grade, year 3*										
2+3	4+5	-0.48	0.87	-1.05	-0.81	1.17	-1.54	-0.81	1.32	-1.79	328 –372
4+5	6+7	-0.02	-1.16	-0.19	-0.05	-1.59	-0.28	-0.05	-1.62	-0.28	284– 337
2	4	1.92	0.32	0.62	2.96	0.45	0.97	2.96	0.54	1.19	164 –188
3	5	-2.04	0.49	-2.72	-3.05	0.65	-3.91	-3.05	0.72	-4.57	164– 184
4	6	2.12	0.41	0.75	2.63	0.53	1.06	2.63	0.57	1.04	167 –196
5	7	-0.77	-0.96	1.21	-1.01	-1.36	1.91	-1.01	-1.21	1.68	117– 141

COMPUTER SOURCE—MPR: sty40304.do, sty40306.do, sty40316.do, sty40318.do-sty40401.do, sty40404.do, sty40501.do, sty40502.do, sty40507.do, sty40508.do.

*Impact is statistically significant at 0.1 level, two-tailed test.
** Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 24

YEAR-ONE, -TWO, AND -THREE READING TEST-SCORE IMPACTS
(Percentile)

		Scholarship Offered			Ever Switched			Years Attended			
		Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)	N (10)
All students											
Overall--average		1.01	1.35	0.27	1.37	1.83	0.39	1.37	1.90	0.43	1,199-1,455
Grade, year 1	*Grade, year 3*										
2+3	4+5	0.16	-0.46	-0.18	0.11	-0.62	-0.25	0.11	-0.65	-0.30	648–766
4+5	6+7	2.46*	3.12*	1.24	3.42*	4.27*	1.75	3.42*	4.41*	1.86	551 –689
2	4	2.19	-1.55	-0.56	3.07	-2.10	-0.82	3.07	-2.23	-1.01	307–370
3	5	-2.79	-0.55	-0.61	-3.93	-0.75	-0.87	-3.93	-0.79	-1.00	341–396
4	6	0.24	2.14	-0.34	0.25	3.00	-0.48	0.25	3.16	-0.52	313 –395
5	7	4.52**	4.67**	3.08	5.93**	6.22**	4.40	5.93**	6.24**	4.51	238 –294
African American students											
Overall--average		3.47**	3.44**	3.97**	4.49**	4.51**	5.45**	4.49**	4.64**	6.66**	497 –623
Grade, year 1	*Grade, year 3*										
2+3	4+5	2.25	1.28	3.35	2.88	1.64	4.62	2.88	1.68	5.66	271 –332
4+5	6+7	5.19***	6.13**	4.64**	6.67***	8.17***	6.17**	6.67***	8.43***	7.63**	226 –291
2	4	5.04	-0.36	3.34	6.72	-0.46	4.62	6.72	-0.46	5.83	118 –152
3	5	-3.73	0.15	0.15	-4.80	0.18	0.20	-4.80	0.19	0.24	153 –180
4	6	2.38	2.43	4.62	3.23	3.44	6.43	3.23	3.38	7.90	122 –167
5	7	8.97***	7.22**	6.27*	10.29***	8.20**	8.04*	10.29***	8.90**	9.66	104 –124
Latino students											
Overall--average		-0.70	0.17	-1.85	-1.01	0.23	-2.73	-1.01	0.25	-2.94	612–709
Grade, year 1	*Grade, year 3*										
2+3	4+5	-0.86	-0.27	-1.52	-1.39	-0.36	-2.22	-1.39	-0.40	-2.59	328–372
4+5	6+7	0.17	1.23	-0.67	0.34	1.66	-1.01	0.34	1.70	-0.99	284 –337
2	4	2.16	-0.13	-0.86	3.27	-0.18	-1.34	3.27	-0.22	-1.66	164–188
3	5	-2.93	-0.53	-2.21	-4.38	-0.71	-3.17	-4.38	-0.78	-3.71	164–184
4	6	0.10	1.75	-2.58	0.08	2.28	-3.64	0.08	2.46	-3.58	167 –196
5	7	2.06	2.14	5.75	2.89	3.02	9.05	2.89	2.69	7.98	117 —141

COMPUTER SOURCE—MPR: sty40304.do, sty40306.do, sty40316.do, sty40318.do, sty40401.do-sty40404.do, sty40501.do, sty40502.do, sty40507.do, sty40508.do.

*Impact is statistically significant at 0.1 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 25

YEAR-ONE, -TWO, AND -THREE MATHEMATICS TEST-SCORE IMPACTS
(Percentile)

		Scholarship Offered			Ever Switched			Years Attended			
		Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)	N (10)
All students											
Overall--average		1.39	-0.43	1.59	1.87	-0.58	2.30	1.87	-0.60	2.56	1,199—1,455
Grade, year 1	*Grade, year 3*										
2+3	4+5	-0.03	-0.93	0.69	-0.05	-1.25	0.99	-0.05	-1.32	1.15	648 –766
4+5	6+7	4.47**	-0.09	2.77*	5.95**	-0.13	3.92*	5.95**	-0.13	4.17*	551 –689
2	4	3.16	-0.24	1.30	4.59	-0.32	1.91	4.59	-0.34	2.36	307 –370
3	5	-2.25	-2.83	0.14	-2.97	-3.85	0.21	-2.97	-4.04	0.24	341- 396
4	6	3.87	-1.05	3.68	5.20	-1.48	5.21	5.20	-1.55	5.55	313 –395
5	7	4.89*	1.08	2.23	6.35*	1.44	3.18	6.35*	1.45	3.26	238 –294
African American students											
Overall--average		5.39***	3.10	7.03***	6.98***	4.07	9.65***	6.98***	4.19	11.80***	497 –623
Grade, year 1	*Grade, year 3*										
2+3	4+5	4.58**	1.40	6.65***	5.95**	1.78	9.17***	5.95**	1.82	11.24***	271 –332
4+5	6+7	7.08***	3.54	6.38***	9.10***	4.71	8.48***	9.10***	4.86	10.48***	226 –291
2	4	8.62	4.05	5.54	11.49	5.20	7.67	11.49	5.11	9.67	118 –152
3	5	0.05	-4.01	5.33**	-0.07	-5.04	6.99**	-0.07	-5.38	8.58*	153- 180
4	6	5.35***	-0.58	4.99	7.25***	-0.83	6.94	7.25***	-0.81	8.53	122 –167
5	7	8.99**	8.62**	8.93**	10.32**	9.80**	11.45**	10.32**	10.64**	13.75**	104- 124
Latino students											
Overall--average		-0.63	-1.37	-0.05	-1.02	-1.87	-0.07	-1.02	-2.02	-0.08	612- 709
Grade, year 1	*Grade, year 3*										
2+3	4+5	-0.09	2.01	-0.58	-0.23	2.69	-0.85	-0.23	3.04	-0.96	328 –372
4+5	6+7	-0.21	-3.54	0.29	-0.44	-4.84	0.44	-0.44	-4.94	0.43	284 –337
2	4	1.68	0.77	2.10	2.64	1.08	3.27	2.64	1.30	4.04	164 –188
3	5	-1.15	1.50	-3.23	-1.72	2.01	-4.64	-1.72	2.22	-5.43	164- 184
4	6	4.13	-0.93	4.08	5.17	-1.22	5.75	5.17	-1.31	5.66	167 –196
5	7	-3.60	-4.07	-3.33	-4.91	-5.74	-5.24	-4.91	-5.11	-4.62	117- 141

COMPUTER SOURCE—MPR: sty40304.do, sty40306.do, sty40316.do, sty40318.do, sty40401.do-sty40404.do, sty40501.do, sty40502.do, sty40507.do, sty40[illegible]5do.

*Impact is statistically significant at 0.1 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE 26

BETWEEN YEAR DIFFERENCES IN IMPACTS[a]

	Composite Score			Reading Score			Math Score			
	Scholarship Offered (1)	Ever Used a Scholarship (2)	Used a Scholarship for 2 years (3)	Scholarship Offered (4)	Ever Used a Scholarship (5)	Used a Scholarship for 2 Years (6)	Scholarship Offered (7)	Ever Used a Scholarship (8)	Used a Scholarship for 2 Years (9)	N (10)
DIFFERENCES BETWEEN YEARS 1 AND 2										
Overall	0.74	1.00	0.97	-0.34	-0.45	-0.53	1.82	2.45	2.47	1199-1455
African American students	1.16	1.44	1.32	0.03	-0.02	-0.15	2.29	2.91	2.79	497-623
Latino students	-0.06	-0.19	-0.13	-0.86	-1.24	-1.25	0.74	0.85	1.00	612-709
DIFFERENCES BETWEEN YEARS 1 AND 3										
Overall	0.27	0.28	0.13	0.74	0.99	0.94	-0.20	-0.43	-0.68	1250-1455
African American students	-1.07	-1.82	-3.49	-0.49	-0.95	-2.16	-1.64	-2.68	-4.82	519-623
Latino students	0.28	0.39	0.50	1.15	1.73	1.93	-0.58	-0.95	-0.94	637-709
DIFFERENCES BETWEEN YEARS 2 AND 3										
Overall	-0.47	-0.72	-0.84	1.08	1.44	1.47	-2.02	-2.88	-3.16*	1199-1250
African American students	-2.23	-3.26*	-4.82**	-0.53	-0.93	-2.02	-3.93**	-5.58**	-7.61***	497-519
Latino students	0.35	0.58	0.62	2.02	2.96	3.19	-1.33	-1.80	-1.94	612-637

COMPUTER SOURCE—MPR: sty40501-sty40502; sty40507-sty40510.

See notes to Table 6.

[a]Appendix C describes how between year differences were tested.

*Impact is statistically significant at .10 level, two tailed test
**Impact is statistically significant at .05 level, two tailed test
***Impact is statistically significant at .01 level., two tailed test

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APPENDIX A

BASELINE CHARACTERISTICS FOR TREATMENT AND CONTROL GROUPS

TABLE A-1

DIFFERENCES IN BASELINE CHARACTERISTICS FOR THIRD FOLLOW-UP
TREATMENTS AND CONTROLS
(MEAN VALUES REPORTED)

Variable	Treatments	Controls	Difference	t-statistic	Significance
Grade of Student (1996–1997)					
Kindergarten	0.17	0.16	0.01	0.31	-
First	0.20	0.20	0.01	0.32	-
Second	0.21	0.22	-0.02	-0.70	-
Third	0.20	0.24	-0.04	-1.59	-
Fourth	0.22	0.17	0.04	1.91	*
Years student attended this school	2.36	2.47	-0.11	-1.51	-
Satisfaction with aspects of current school					
Location	3.03	2.94	0.08	1.73	*
School safety	2.80	2.76	0.04	0.90	-
Teaching	2.66	2.69	-0.03	-0.63	-
How much school involves parents	2.73	2.72	0.01	0.27	-
Class sizes	2.34	2.33	0.00	0.06	-
School facilities	2.60	2.64	-0.05	-0.99	-
Student respect of teachers	2.89	2.86	0.02	0.51	-
Parent-teacher communication	2.81	2.82	0.00	-0.04	-
Observance of religious traditions	2.24	2.33	-0.09	-1.71	*
Student in gifted classes	0.11	0.13	-0.02	-1.27	-
Student received help for disability	0.12	0.10	0.02	1.15	-
Mother's educational expectations for child (10=some high school, 12=high school graduation, 14=some college, 16=college graduation, 18=more than college)	16.73	16.69	0.04	0.43	-
Education level of mother or female guardian					
Some high school (did not graduate)	0.21	0.21	0.00	0.06	-
High school graduate or GED certificate	0.25	0.28	-0.03	-1.06	-
Some college	0.41	0.40	0.01	0.40	-
Graduated from four-year college	0.08	0.07	0.01	0.73	-
More than four-year college degree	0.02	0.03	-0.01	-0.61	-
Don't know	0.02	0.01	0.01	1.32	-

TABLE A-1 *(continued)*

Variable	Treatments	Controls	Difference	t-statistic	Significance
Race/ethnicity of mother/female guardian					
White	0.05	0.04	0.01	0.92	-
Black	0.48	0.44	0.03	1.19	-
Puerto Rican	0.17	0.20	-0.03	-1.40	-
Hispanic other than Puerto Rican	0.25	0.28	-0.03	-1.13	-
Other	0.05	0.04	0.01	1.26	-
Birth place of mother/female guardian					
Born in United States	0.60	0.62	-0.02	-0.59	-
Born in Puerto Rico	0.07	0.08	-0.01	-0.77	-
Born outside U.S. and Puerto Rico	0.32	0.29	0.03	1.08	-
Length of residence of mother in months	36.07	36.50	-0.44	-0.65	-
Job status of mother/female guardian					
Full-time job	0.23	0.20	0.03	1.15	-
Part-time job	0.15	0.15	0.00	0.23	-
Not working now but looking for work	0.46	0.46	0.00	-0.07	-
Not working and not looking for work	0.14	0.16	-0.02	-0.92	-
Don't know	0.01	0.02	-0.01	-1.42	-
Religious affiliation of female guardian					
Catholic	0.50	0.54	-0.04	-1.39	-
Religion other than Catholic	0.45	0.41	0.04	1.41	-
None	0.05	0.05	0.00	-0.02	-
Number of children in home	2.41	2.44	-0.02	-0.30	-
In child's home (percent saying yes)					
A daily newspaper	0.85	0.84	0.01	0.67	-
An encyclopedia	0.70	0.70	0.00	-0.13	-
A dictionary	0.97	0.98	0.00	-0.17	-
More than 50 books	0.86	0.85	0.01	0.35	-
Member of household receiving assistance					
Food stamps	0.66	0.67	-0.01	-0.40	-
Welfare	0.56	0.58	-0.02	-0.75	-
Social Security	0.11	0.11	0.00	0.12	-
Medicaid	0.62	0.67	-0.05	-1.76	*
Supplemental Security Income	0.14	0.14	-0.01	-0.32	-
Family income	9577.01	9533.28	43.74	0.11	-

TABLE A-1 *(continued)*

Variable	Treatments	Controls	Difference	t-statistic	Significance
Reading achievement scores					
Overall	23.10	25.36	-2.26	-1.59	-
First-grade cohort	22.08	29.01	-6.93	-2.15	**
Second-grade cohort	26.54	26.04	0.50	0.17	-
Third-grade cohort	19.29	22.63	-3.35	-1.47	-
Fourth-grade cohort	24.68	24.23	0.45	0.16	-
Mathematics achievement scores					
Overall	17.85	17.66	0.19	0.15	-
First-grade cohort	9.78	11.72	-1.94	-1.04	-
Second-grade cohort	21.14	19.42	1.72	0.73	-
Third-grade cohort	16.09	18.35	-2.26	-0.95	-
Fourth-grade cohort	24.46	20.79	3.68	1.18	-
English spoken at home	0.78	0.75	0.03	1.35	-

COMPUTER SOURCE—MPR: fay40101.do, fa40101a.ado.

*Significantly different from zero at the .10 level.
**Significantly different from zero at the .05 level.
***Significantly different from zero at the .01 level.

TABLE A-2

DIFFERENCES IN BASELINE CHARACTERISTICS FOR RESPONDENTS AND NONRESPONDENTS IN THIRD FOLLOW-UP: TREATMENT GROUP (MEAN VALUES REPORTED)

Variable	Respond-ents	Non-respondents	Difference	t-statistic	Significance
Grade of Student (1996–1997)					
Kindergarten	0.16	0.13	0.03	1.08	-
First	0.19	0.18	0.01	0.24	-
Second	0.21	0.21	0.00	0.15	-
Third	0.22	0.27	-0.04	-1.44	-
Fourth	0.21	0.21	0.00	0.05	-
Years student attended this school	2.43	2.45	-0.02	-0.23	-
Satisfaction with aspects of current school					
Location	3.04	2.92	0.12	1.93	*
School safety	2.80	2.67	0.13	2.08	**
Teaching	2.68	2.54	0.13	2.15	**
How much school involves parents	2.72	2.61	0.11	1.87	*
Class sizes	2.33	2.33	0.00	-0.06	-
School facilities	2.61	2.53	0.09	1.53	-
Student respect of teachers	2.89	2.78	0.12	1.94	*
Parent-teacher communication	2.84	2.71	0.14	2.28	**
Observance of religious traditions	2.26	2.21	0.06	0.81	-
Student in gifted classes	0.11	0.13	-0.02	-0.84	-
Student received help for disability	0.11	0.14	-0.03	-1.17	-
Mother's educational expectations for child (10=some high school, 12= high school graduation, 14=some college, 16=college graduation, 18=more than college)	16.77	16.58	0.20	1.52	-
Education level of mother or female guardian					
Some high school (did not graduate)	0.23	0.23	0.01	0.18	-
High school graduate or GED certificate	0.24	0.25	-0.01	-0.21	-
Some college	0.41	0.39	0.01	0.38	-
Graduated from four-year college	0.07	0.08	-0.01	-0.45	-
More than four-year college degree	0.03	0.03	-0.01	-0.54	-
Don't know	0.02	0.01	0.00	0.32	-
Race/ethnicity of mother/female guardian					
White	0.03	0.08	-0.05	-2.74	***
Black	0.45	0.52	-0.07	-1.92	*
Puerto Rican	0.18	0.19	-0.01	-0.47	-
Hispanic other than Puerto Rican	0.29	0.18	0.11	3.79	***
Other	0.05	0.03	0.02	1.44	-

TABLE A-2 *(Continued)*

Variable	Respond-ents	Non-respondents	Difference	t-statistic	Significance
Birth place of mother/female guardian					
Born in United States	0.57	0.64	-0.07	-1.92	*
Born in Puerto Rico	0.07	0.09	-0.02	-0.86	-
Born outside U.S. and Puerto Rico	0.35	0.27	0.09	2.54	**
Length of residence of mother in months	36.52	35.11	1.41	1.57	-
Job status of mother/female guardian					
Full-time job	0.22	0.20	0.02	0.76	-
Part-time job	0.16	0.16	-0.01	-0.27	-
Not working now but looking for work	0.45	0.49	-0.03	-0.87	-
Not working and not looking for work	0.16	0.14	0.01	0.49	-
Don't know	0.01	0.01	0.00	0.53	-
Religious affiliation of female guardian					
Catholic	0.53	0.46	0.07	1.89	*
Religion other than Catholic	0.43	0.48	-0.06	-1.60	-
None	0.05	0.06	-0.01	-0.66	-
Number of children in home	2.45	2.36	0.09	1.02	-
In child's home (percent saying yes)					
A daily newspaper	0.84	0.86	-0.02	-0.70	-
An encyclopedia	0.69	0.72	-0.02	-0.66	-
A dictionary	0.97	0.97	0.01	0.50	-
More than 50 books	0.84	0.85	-0.01	-0.30	-
Member of household receiving assistance					
Food stamps	0.65	0.72	-0.07	-1.98	**
Welfare	0.54	0.65	-0.11	-3.03	***
Social Security	0.12	0.12	0.00	0.00	-
Medicaid	0.63	0.68	-0.06	-1.64	-
Supplemental Security Income	0.14	0.17	-0.03	-1.03	-
Family income	9821.59	8774.81	1046.78	2.17	**

TABLE A-2 *(Continued)*

Variable	Respond-ents	Non-respondents	Difference	t-statistic	Significance
Reading achievement scores					
Overall	22.74	21.01	1.73	1.02	-
First-grade cohort	22.48	23.62	-1.14	-0.27	-
Second-grade cohort	24.62	20.17	4.44	1.12	-
Third-grade cohort	19.11	20.21	-1.11	-0.42	-
Fourth-grade cohort	25.17	20.67	4.49	1.44	-
Math achievement scores					
Overall	17.66	15.29	2.37	1.49	-
First-grade cohort	10.06	9.22	0.84	0.31	-
Second-grade cohort	20.22	18.62	1.60	0.54	-
Third-grade cohort	17.86	13.47	4.38	1.64	-
Fourth-grade cohort	21.24	19.44	1.80	0.45	-
English spoken at home	0.74	0.83	-0.09	-2.98	***

COMPUTER SOURCE—MPR: fay40101.do, fay40101.ado.

*Significantly different from zero at the .10 level.
**Significantly different from zero at the .05 level.
***Significantly different from zero at the .01 level.

TABLE A-3

DIFFERENCES IN BASELINE CHARACTERISTICS FOR RESPONDENTS AND NONRESPONDENTS IN THIRD FOLLOW-UP: CONTROL GROUP (MEAN VALUES REPORTED)

Variable	Respondents	Non-respondents	Difference	t-statistic	Significance
Grade of Student (1996–1997)					
Kindergarten	0.17	0.14	0.04	1.41	-
First	0.18	0.15	0.03	1.17	-
Second	0.22	0.21	0.01	0.17	-
Third	0.24	0.25	-0.01	-0.30	-
Fourth	0.19	0.25	-0.06	-2.03	**
Years student attended this school	2.45	2.60	-0.15	-1.57	-
Satisfaction with aspects of current school					
Location	2.92	2.86	0.06	0.89	-
School safety	2.73	2.65	0.08	1.17	-
Teaching	2.71	2.57	0.14	2.17	**
How much school involves parents	2.74	2.62	0.12	2.00	**
Class sizes	2.34	2.23	0.11	1.66	*
School facilities	2.64	2.51	0.13	2.01	**
Student respect of teachers	2.86	2.74	0.12	1.77	*
Parent-teacher communication	2.82	2.72	0.10	1.47	-
Observance of religious traditions	2.31	2.23	0.08	1.19	-
Student in gifted classes	0.11	0.12	-0.01	-0.26	-
Student received help for disability	0.11	0.11	0.00	-0.10	-
Mother's educational expectations for child (10=some high school, 12=high school graduation, 14=some college, 16=college graduation, 18=more than college)	16.64	16.80	-0.16	-1.31	-
Education level of mother or female guardian					
Some high school (did not graduate)	0.24	0.20	0.03	1.09	-
High school graduate or GED certificate	0.28	0.28	0.00	0.04	-
Some college	0.39	0.40	-0.01	-0.27	-
Graduated from four-year college	0.06	0.08	-0.03	-1.39	-
More than four-year college degree	0.03	0.02	0.00	0.25	-
Don't know	0.01	0.01	0.00	-0.01	-
Race/ethnicity of mother/female guardian					
White	0.03	0.04	-0.01	-0.50	-
Black	0.42	0.48	-0.06	-1.70	*
Puerto Rican	0.22	0.22	-0.01	-0.18	-
Hispanic other than Puerto Rican	0.31	0.23	0.08	2.52	**
Other	0.03	0.04	-0.01	-0.38	-

TABLE A-3 *(Continued)*

Variable	Respondents	Non-respondents	Difference	t-statistic	Significance
Birth place of mother/female guardian					
Born in United States	0.62	0.61	0.00	0.08	-
Born in Puerto Rico	0.09	0.09	0.01	0.25	-
Born outside U.S. and Puerto Rico	0.29	0.30	-0.01	-0.28	-
Length of residence of mother in months	36.97	35.20	1.77	1.95	*
Job status of mother/female guardian					
Full-time job	0.20	0.25	-0.05	-1.53	-
Part-time job	0.13	0.18	-0.05	-1.67	*
Not working now but looking for work	0.48	0.41	0.07	1.99	**
Not working and not looking for work	0.16	0.15	0.01	0.44	-
Don't know	0.03	0.02	0.01	0.95	-
Religious affiliation of female guardian					
Catholic	0.57	0.49	0.08	2.04	**
Religion other than Catholic	0.39	0.45	-0.06	-1.64	-
None	0.05	0.06	-0.02	-0.88	-
Number of children in home	2.48	2.32	0.16	1.68	*
In child's home (percent saying yes)					
A daily newspaper	0.84	0.86	-0.02	-0.76	-
An encyclopedia	0.70	0.73	-0.03	-0.89	-
A dictionary	0.98	0.97	0.01	0.93	-
More than 50 books	0.86	0.81	0.05	1.66	*
Member of household receiving assistance					
Food stamps	0.69	0.66	0.03	0.77	-
Welfare	0.61	0.57	0.04	0.99	-
Social Security	0.13	0.07	0.06	2.62	***
Medicaid	0.69	0.62	0.07	1.89	*
Supplemental Security Income	0.15	0.12	0.03	1.10	-
Family income	9,007.05	10,097.17	-1,090.11	-1.97	**
Reading achievement scores					
Overall	23.26	25.35	-2.09	-1.16	-
First-grade cohort	26.95	31.27	-4.32	-1.03	-
Second-grade cohort	23.92	24.68	-0.76	-0.19	-
Third-grade cohort	20.01	21.95	-1.94	-0.70	-
Fourth-grade cohort	23.07	25.22	-2.15	-0.61	-

TABLE A-3 *(Continued)*

Variable	Respondents	Non-respondents	Difference	t-statistic	Significance
Math achievement scores					
Overall	16.63	18.84	-2.21	-1.29	-
First-grade cohort	11.43	13.88	-2.45	-0.88	-
Second-grade cohort	18.43	19.91	-1.49	-0.43	-
Third-grade cohort	15.58	19.03	-3.46	-1.10	-
Fourth-grade cohort	20.89	21.24	-0.35	-0.09	-
English spoken at home	0.73	0.82	-0.09	-2.84	***

COMPUTER SOURCE—MPR: fay40101.do, fay40101.ado.

*Significantly different from zero at the .10 level.
**Significantly different from zero at the .05 level.
***Significantly different from zero at the .01 level.

APPENDIX B

ADJUSTING SAMPLE WEIGHTS FOR NONRESPONSE

ADJUSTING SAMPLE WEIGHTS FOR NONRESPONSE

Families within the sample had different probabilities of being offered a scholarship. To reflect these differences in the probability of selection and to reflect the composition of the population of eligible applicants, we weighted the sample data. The weights were constructed by taking the inverse of the probability of being selected for a scholarship. Weights for scholarship families were multiplied by .217 and weights for control-group families by .783 to reflect the ratio of treatment to control-group families in the initial pool of eligible applicants. All weights were divided by 2 to sum to the size of the population we were trying to represent, not twice the population. In this sample, the average weight was about 4.2. A family with a weight of 4.2 stands in for 3.2 other families in the pool of applicants as well as itself. The weights, which were adjusted for the same family applying multiple times, range in size from about .5 to 22.

About 18 percent of all families in the first year, 26 percent in the second year, and 28 percent in the third year did not complete a survey. To adjust for such nonresponse, we computed the probability of responding based on a logit model.[23] The independent variables in the logit model included family characteristics such as race/ethnicity, number of siblings, language spoken at home, mother's education, family income, and other variables used to stratify the sample when we collected the baseline data. After computing the predicted probability of responding, we adjusted the baseline weight as follows:

$$W_i = 1/[f_i * p_i * pr_i],$$

where f_i includes the adjustment factors used for deriving the baseline weight[24], p_i is the probability of being selected for a scholarship (control group), pr_i is the probability of responding to a follow-up survey, and W_i is the new weight variable. Families that did not respond to the follow-up survey were assigned a weight of zero.

For the third-year student data, we found that 68 percent of the students responded to the survey and that we had test scores for 67 percent. To adjust the weights for the student-level data, we followed the same procedures used for the parent data.

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[23] Weights correspond only to the presence or absence of a family/student at a point in time and do not require that families or students are present in the sample for more than a single data session.

[24] The adjustment factors are as follows: (1) five discrete points at which families applied for scholarships; (2) whether a child attended a public school with below-average achievement; and (3) the number of eligible children within the family (see Peterson, Myers, Haimson, and Howell 1997).

APPENDIX C

ANALYTIC APPROACH

ANALYTIC APPROACH

For some analysts and program operators, the important policy question is as follows: what happens when a voucher or scholarship program is put into effect? But other analysts also want an answer to a second question: what is the impact of attending private school? Angrist, Imbens, and Rubin (1996) make some interesting distinctions between the two estimators used to address these two different questions. We refer to the first estimator as the intended to treat (ITT) estimator and the second as the complier average causal effect estimator (CACE). The most important issues concerning the ITT estimator, which compares all children randomly assigned to the scholarship group with all children randomly assigned to the control group, are that among the children assigned to the two groups are (1) children who are induced by the offer of a scholarship to attend private school, (2) children who would have made the decision to attend private school regardless of the scholarship offer, and (3) children who would never attend private school. The CACE estimator provides an estimate of the impact of the scholarship for only those who were or would have been induced by the offer of a scholarship to attend private school. Children who would have attended private school regardless of the offer of a scholarship and those who would have opted not to attend irrespective of the scholarship do not play a direct role in the estimated impact with the CACE estimator.

Computing Impacts of Being Offered a Scholarship (ITT)

To compute the impact of being offered a scholarship, we use a simple statistical model that includes as independent variables an indicator for treatment status (offered a scholarship or in the control group) and a set of indicators that show the stratum from which a family was selected.[25] The strata are based on (1) five discrete points at which families applied for scholarships, (2) whether a child attended a public school with below-average achievement, and (3) the number of eligible children within the family. When computing the impact on student achievement test scores, we also included student baseline reading and mathematics achievement as independent variables. The basic form of the model is:

$$y_{3i} = \beta_0 + \beta_1 T_i + \beta_2 X_i + \varepsilon_{3i}, \qquad \text{(C.1)}$$

where y_{3i} is the outcome as measured in year three for respondent i; T_i equals 1 if we offered a family a scholarship and 0 otherwise (families were randomly selected for the scholarship and control groups); X_i is a vector that includes indicator variables for each of the strata used in the random selection of scholarship families and baseline test scores when computing impacts on achievement; the random error term ε_{3i} captures the effects of unobserved factors that influence the outcome; and the βs are parameters or vectors of parameters to be estimated. The parameter of most interest is β_1 because it shows the impact of being offered a scholarship on the outcome

[25] We estimate impacts for two points in time so we can assess change in impacts.

for year three. We estimate the model parameters by using ordinary least squares for both categorical and continuous outcomes. Standard errors are computed by using the bootstrap. For a few binary outcomes, we checked the robustness of the ordinary least squares estimates by estimating a logit model. Similar qualitative conclusions held regardless of the method of estimation.

Computing the Complier Average Causal Effect (CACE)

A simple comparison of an outcome for families in the scholarship group (those to whom we offered a scholarship) and the control group shows the impact of *being offered a scholarship* regardless of whether or not a family sent its child to private school.

To compute the CACE estimator for the impact of *ever* attending private school, we need to estimate a statistical model that focuses on (1) the relationship between being offered a scholarship and *ever* attending private school and (2) the relationship between *ever* attending a private school and family and student outcomes. These relationships can be expressed as:

$$\begin{aligned} P_{3i} &= \alpha_0 + \alpha_1 T_i + \alpha_2 X_i + \varepsilon_{p3i} \\ y_{3i} &= \beta_0 + \beta_1 P_{3i} + \beta_2 X_i + \varepsilon_{y3i} \end{aligned} \qquad \text{(C.2)}$$

where T_i equals 1 if we offered a family a scholarship and 0 otherwise (families were randomly selected for the scholarship and control groups); X_i is a vector that includes indicator variables for each of the strata used in the random selection of scholarship families and baseline test scores when computing impacts on achievement; P_{3i} equals 1 if ever attended private school and 0 otherwise; y_{3i} is the outcome of interest; ε_{p3i} and ε_{y3i} are random error terms that capture the effects of unobserved factors that influence both private-school attendance and the outcome; and α's and β's are parameters or vectors of parameters to be estimated.[26] The parameter of most interest is β_1 because it shows the impact of ever attending private school on the outcome.[27]

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[26] For analyses of the parent and student survey data, we focused on attendance at private school in year three only. In this case, P_{3i} = 1 if attended private school in year three and 0 otherwise.

[27] As already described in the report, we used two definitions of private-school attendance when analyzing the test scores: (1) ever attend private school and (2) attended private school for three years. The impacts for attending for three years are implemented by making a small adjustment to the analytic models, which entails setting P_{3i} = 1 if a student attended private school for three years and0 otherwise. When using the instrumental variables estimator to estimate the impact of ever attending private school, we must assume that the scholarship offer had no impact on the year-three test scores for students who did not use a scholarship. To use the instrumental variables estimator to estimate the impact of attending private school for three years, we must make the same assumption as above and assume that the exposure to private schooling among students who attended for one or two years had no impact on the third-year test scores.

We estimate the model parameters by using the instrumental variables (IV) estimator. This technique allows us to compute asymptotically unbiased and efficient estimates of the parameters, which can be interpreted as the causal impact for compliers (students who were induced ever to attend private school by the scholarship offer) by using the framework developed by Angrist, Imbens, and Rubin, 1996. To implement the instrumental variables estimator, we use the two-stage least squares procedure.

In our tables we present (1) the impact of switching to private school, (2) the average of each outcome for families or students in the scholarship group who switched to private school (complied with the treatment protocol), and (3) the average of outcomes for families or students in the control group who would have attended private school if offered a scholarship. The first quantity is obtained from the statistical model described previously. The average for compliers in the control group is computed by adding the impact of attending private school to the average for members of the treatment group who would have complied. To compute the last quantity, we can use an alternative expression for computing the impacts of private-school attendance (compliance):

$$E(y^T_{,}/COMP=1) = [E(y^T_{,}) - E(y^T_{,}/A=1)\bullet Pr(A=1) - E(y^T_{,}/N=1)\bullet Pr(N=1)]\bullet Pr(COMP=1)^{-1}$$

where

$E(y^T_{,}/COMP=1)$	=	mean achievement at time = 3 for compliers (COMP = 1)
$E(y^T_{,})$	=	overall mean for the treatment group
$E(y^T_{,}/A=1)$	=	mean for students who would always attend private school regardless of the voucher offer
$E(y^T_{,}/N=1)$	=	mean for students who would never attend private school regardless of the voucher offer
$Pr\ (A=1)$	=	probability of always attending private school
$Pr\ (N=1)$	=	probability of never attending private school
$Pr\ (COMP=1)$	=	probability of complying

The above expression tells us that the average of each outcome for compliers, which is unobserved, can be computed from known quantities from the treatment group and control group.

Model Specification for Looking at Grade-Specific Impacts and Between-Year Impacts on Reading and Mathematics Achievement

Our analyses examined three hypotheses:

- Average impacts on student and family outcomes were the same in years one, two, and three (and in year one and year two, and in year two and year three).
- Grade-specific impacts on students' reading and mathematics achievement test scores were similar within year one, year two, and year three.
- Grade-specific impacts on students' reading and mathematics achievement test scores were similar across years.

To test these hypotheses, we constructed functions of the impact estimates and computed the standard errors of the functions by using the bootstrap method. Tables C-1 and C-2 list the specific functions. To implement our across-time and across- cohort analysis, we estimate equations similar to C.1 and C.2 for earlier periods.

For several reasons, we used the bootstrap to compute direct estimates of the standard errors. First, some analyses involve more than one child from each family, which produces clustering in the sample. To adjust for the clustering, we sampled families instead of children when constructing the bootstrap samples. Second, the estimation of private-school impacts involved the use of the IV estimator and is complicated by the implicit presence of interaction terms in the model when comparing across-time or between-cohort impact estimates. To make these comparisons, we computed the functions in Tables C-1 and C-2 for each bootstrap sample and then computed the standard errors of the functions after 1,000 samples were formed and the models and functions were estimated. To assess whether overall impacts and grade-specific impacts changed between years, we computed some additional estimates, also indicated in Table C-2.

To statistically test the hypotheses that involved making multiple comparisons, we used the Bonferonni procedure. The Bonferonni allows us to control the probability of making a type 1 error when making multiple comparisons. To use the Bonferonni, we can take the probability of making a type 1 error for a z-test, for example, and divide it by the number of comparisons made. For example, if the probability is 0.10 and we are making four comparisons, then the critical value used for each comparison should be the value associated with a type 1 error of .025. By dividing by the number of planned comparisons, we implicitly set the probability of making one or more type 1 errors among the planned comparisons in the set to 0.10. This should be about equivalent to using an F-test in the usual setting when we want to test for differences between/among two or more means.[28]

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[28] Similar procedures were used to assess change between years one and two and years two and three as well.

TABLE C-1

FUNCTIONS OF IMPACT ESTIMATES FOR GRADE-SPECIFIC IMPACTS WITHIN YEARS[a]

Parameters of Interest	Interpretation
$\Delta_{11} = I_{11} - I_{21}$	Difference in impacts for grades two and three—year one
$\Delta_{21} = I_{11} - I_{31}$	Difference in impacts for grades two and four—year one
$\Delta_{31} = I_{11} - I_{41}$	Difference in impacts for grades two and five—year one
$\Delta_{41} = I_{21} - I_{31}$	Difference in impacts for grades three and four—year one
$\Delta_{51} = I_{21} - I_{41}$	Difference in impacts for grades three and five—year one
$\Delta_{61} = I_{31} - I_{41}$	Difference in impacts for grades four and five—year one
$\Delta_{12} = I_{12} - I_{22}$	Difference in impacts for grades three and four—year two
$\Delta_{22} = I_{12} - I_{32}$	Difference in impacts for grades three and five—year two
$\Delta_{32} = I_{12} - I_{42}$	Difference in impacts for grades three and six—year two
$\Delta_{42} = I_{22} - I_{32}$	Difference in impacts for grades four and five—year two
$\Delta_{52} = I_{22} - I_{42}$	Difference in impacts for grades four and six—year two
$\Delta_{62} = I_{32} - I_{42}$	Difference in impacts for grades five and six—year two
$\Delta_{13} = I_{13} - I_{23}$	Difference in impacts for grades four and five—year three
$\Delta_{23} = I_{13} - I_{33}$	Difference in impacts for grades four and —six year three
$\Delta_{33} = I_{13} - I_{43}$	Difference in impacts for grades four and seven—year three
$\Delta_{43} = I_{23} - I_{33}$	Difference in impacts for grades five and six—year three
$\Delta_{53} = I_{23} - I_{43}$	Difference in impacts for grades five and seven—year three
$\Delta_{63} = I_{33} - I_{43}$	Difference in impacts for grades six and seven—year three

[a]Grade-specific impacts refer only to analyses of achievement-test score impacts and not to analyses of family and student survey data.

TABLE C-2

FUNCTIONS OF IMPACT ESTIMATES FOR
BETWEEN-YEAR COMPARISONS

Parameters of Interest	Interpretation
$\Delta_1 = I_{11} - I_{12}$	Difference in impacts for years one and two for grade three
$\Delta_2 = I_{21} - I_{22}$	Difference in impacts for years one and two for grade four
$\Delta_3 = I_{31} - I_{32}$	Difference in impacts for years one and two for grade five
$\Delta_4 = I_{41} - I_{42}$	Difference in impacts for years one and two for grade six
$\Delta_5 = I_{Overall1} - I_{Overall2}$	Difference in impacts for years one and two overall
$\Delta_6 = I_{11} - I_{13}$	Difference in impacts for years one and three for grade four
$\Delta_7 = I_{21} - I_{23}$	Difference in impacts for years one and three for grade five
$\Delta_8 = I_{31} - I_{33}$	Difference in impacts for years one and three for grade six
$\Delta_9 = I_{41} - I_{43}$	Difference in impacts for years one and three for grade seven
$\Delta_{10} = I_{Overall1} - I_{Overall3}$	Difference in impacts for years one and three overall
$\Delta_{11} = I_{12} - I_{13}$	Difference in impacts for years two and three for grade four
$\Delta_{12} = I_{22} - I_{23}$	Difference in impacts for years two and three for grade five
$\Delta_{13} = I_{32} - I_{33}$	Difference in impacts for years two and three for grade six
$\Delta_{14} = I_{42} - I_{43}$	Difference in impacts for years two and three for grade seven
$\Delta_{15} = I_{Overall2} - I_{Overall3}$	Difference in impacts for years two and three overall

NOTE: Grade reported is the students' grade at the latest year used in the difference calculation. For example, the difference in impacts between years one and three for students in grade four refers to the cohort that was in grade two in year one and grade four in year three.

APPENDIX D

TEST-SCORE DIFFERENCES BETWEEN COHORTS

TABLE D-1

YEAR ONE, TWO, AND THREE COMPOSITE TEST SCORE DIFFERENCES BETWEEN COHORTS
(Percentile)

Differences between cohorts	Scholarship Offered			Ever Switched			Years Attended		
	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)
All students									
1+2 and 3+4	-3.40*	-2.21	-1.75	-4.66*	-3.01	-2.47	-4.66*	-3.12	-2.58
1 and 2	5.19**	0.80	0.61	7.28*	1.09	0.88	7.28*	1.13	1.06
1 and 3	0.61	-1.44	-1.30	1.11	-1.98	-1.81	1.11	-2.09	-1.84
1 and 4	-2.03	-3.77	-2.28	-2.31	-5.04	-3.25	-2.31	-5.13	-3.21
2 and 3	-4.58*	-2.23	-1.91	-6.18*	-3.07	-2.69	-6.18*	-3.22	-2.90
2 and 4	-7.23***	-4.57*	-2.89	-9.59***	-6.13*	-4.12	-9.59***	-6.26*	-4.27
3 and 4	-2.65	-2.33	-0.98	-3.42	-3.07	-1.43	-3.42	-3.04	-1.37
African-American students									
1+2 and 3+4	-2.72	-3.49	-0.51	-3.47	-4.73	-0.43	-3.47	-4.89	-0.61
1 and 2	8.67**	3.78	1.70	11.54**	4.80	2.55	11.54**	4.92	3.34
1 and 3	2.97	0.92	-0.37	3.86	1.06	-0.54	3.86	1.04	-0.47
1 and 4	-2.15	-6.07	-3.17	-1.20	-6.63	-3.60	-1.20	-7.44	-3.96
2 and 3	-5.70*	-2.86	-2.06	-7.68*	-3.74	-3.09	-7.68*	-3.87	-3.81
2 and 4	-10.82***	-9.85***	-4.86	-12.74***	-11.43***	-6.15	-12.74***	-12.37***	-7.30
3 and 4	-5.12	-7.00*	-2.80	-5.07	-7.69	-3.06	-5.07	-8.49	-3.49
Hispanic students									
1+2 and 3+4	-0.46	2.03	-0.86	-0.76	2.75	-1.25	-0.76	2.94	-1.52
1 and 2	3.96	-0.17	3.34	6.01	-0.20	4.87	6.01	-0.18	5.76
1 and 3	-0.20	-0.09	-0.13	0.33	-0.08	-0.09	0.33	-0.03	0.15
1 and 4	2.69	1.28	-0.59	3.97	1.81	-0.94	3.97	1.75	-0.49
2 and 3	-4.17	0.08	-3.47	-5.68	0.12	-4.96	-5.68	0.14	-5.61
2 and 4	-1.27	1.45	-3.93	-2.04	2.01	-5.82	-2.04	1.92	-6.25
3 and 4	2.89	1.37	-0.46	3.64	1.89	-0.85	3.64	1.78	-0.64

COMPUTER SOURCE—MPR: sty40501.do, sty40502.do, sty40507.do, sty40508.do, sty40513.do-sty40516.do.

*Impact is statistically significant at 0.1 level, two tailed test.
**Impact is statistically at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE D-2

YEAR ONE, TWO, AND THREE READING TEST SCORE DIFFERENCES BETWEEN COHORTS
(Percentile)

Differences between cohorts	Scholarship Offered			Ever Switched			Years Attended		
	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)
All students									
1+2 and 3+4	-2.30	-3.58*	-1.41	-3.31	-4.89*	-2.00	-3.31	-5.06*	-2.15
1 and 2	4.98	-1.00	0.06	7.00	-1.35	0.05	7.00	-1.44	-0.01
1 and 3	1.94	-3.69	-0.22	2.82	-5.11	-0.33	2.82	-5.38	-0.49
1 and 4	-2.34	-6.23**	-3.64	-2.86	-8.32**	-5.21	-2.86	-8.47**	-5.51
2 and 3	-3.03	-2.69	-0.27	-4.18	-3.75	-0.39	-4.18	-3.94	-0.49
2 and 4	-7.31***	-5.22*	-3.69	-9.86***	-6.97*	-5.27	-9.86***	-7.03*	-5.51
3 and 4	-4.28	-2.54	-3.42	-5.68	-3.22	-4.88	-5.68	-3.09	-5.02
African-American students									
1+2 and 3+4	-2.94	-4.84	-1.29	-3.79	-6.53	-1.54	-3.79	-6.75	-1.96
1 and 2	8.77*	-0.51	3.19	11.53*	-0.65	4.43	11.53*	-0.65	5.58
1 and 3	2.66	-2.80	-1.28	3.50	-3.91	-1.80	3.50	-3.83	-2.07
1 and 4	-3.92	-7.58	-2.94	-3.57	-8.66	-3.42	-3.57	-9.36	-3.83
2 and 3	-6.11*	-2.29	-4.47	-8.03*	-3.26	-6.23	-8.03*	-3.18	-7.65
2 and 4	-12.69***	-7.07*	-6.12	-15.10***	-8.02*	-7.84	-15.10***	-8.71	-9.42
3 and 4	-6.59	-4.78	-1.66	-7.06	-4.76	-1.61	-7.06	-5.53	-1.76
Hispanic students									
1+2 and 3+4	-1.03	-1.48	-0.85	-1.73	-2.02	-1.21	-1.73	-2.10	-1.60
1 and 2	5.09	0.40	1.35	7.65	0.52	1.83	7.65	0.56	2.05
1 and 3	2.05	-1.88	1.72	3.19	-2.47	2.29	3.19	-2.68	1.92
1 and 4	0.10	-2.27	-6.61	0.38	-3.20	-10.40	0.38	-2.91	-9.64
2 and 3	-3.03	-2.28	0.37	-4.46	-2.99	0.46	-4.46	-3.24	-0.13
2 and 4	-4.99	-2.67	-7.96	-7.27	-3.73	-12.23	-7.27	-3.47	-11.69
3 and 4	-1.95	-0.39	-8.33*	-2.81	-0.74	-12.69	-2.81	-0.23	-11.56

COMPUTER SOURCE—MPR: sty40501.do, sty40502.do, sty40507.do, sty40508.do, sty40513.do-sty40516.do.

*Impact is statistically significant at 0.1 level, two tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

TABLE D-3

YEAR ONE, TWO, AND THREE MATH TEST SCORE DIFFERENCES BETWEEN COHORTS
(Percentile)

Differences between cohorts	Scholarship Offered			Ever Switched			Years Attended		
	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 1 (4)	Year 2 (5)	Year 3 (6)	1 Year (7)	2 Years (8)	3 Years (9)
All students									
1+2 and 3+4	-4.50*	-0.84	-2.08	-6.01*	-1.12	-2.93	-6.01*	-1.18	-3.01
1 and 2	5.41*	2.59	1.16	7.56*	3.53	1.70	7.56*	3.70	2.12
1 and 3	-0.71	0.81	-2.38	-0.61	1.16	-3.30	-0.61	1.21	-3.20
1 and 4	-1.73	-1.32	-0.92	-1.76	-1.76	-1.28	-1.76	-1.78	-0.91
2 and 3	-6.12*	-1.78	-3.54	-8.17*	-2.38	-5.00	-8.17*	-2.49	-5.32
2 and 4	-7.14**	-3.91	-2.08	-9.33**	-5.29	-2.98	-9.33**	-5.49	-3.03
3 and 4	-1.02	-2.13	1.46	-1.15	-2.92	2.02	-1.15	-3.00	2.29
African American students									
1+2 and 3+4	-2.50	-2.14	0.27	-3.15	-2.93	0.69	-3.15	-3.03	0.75
1 and 2	8.57**	8.06	0.21	11.55**	10.25	0.68	11.55**	10.49	1.09
1 and 3	3.27	4.64	0.55	4.23	6.03	0.73	4.23	5.92	1.14
1 and 4	-0.38	-4.57	-3.39	1.16	-4.59	-3.77	1.16	-5.53	-4.08
2 and 3	-5.30	-3.42	0.34	-7.32	-4.22	0.05	-7.32	-4.57	0.04
2 and 4	-8.95*	-12.63***	-3.60	-10.39*	-14.84***	-4.45	-10.39*	-16.02***	-5.18
3 and 4	-3.65	-9.21*	-3.94	-3.07	-10.62	-4.50	-3.07	-11.45	-5.22
Hispanic students									
1+2 and 3+4	0.11	5.54	-0.88	0.21	7.52	-1.29	0.21	7.98	-1.43
1 and 2	2.83	-0.73	5.33	4.36	-0.92	7.92	4.36	-0.91	9.47
1 and 3	-2.45	1.70	-1.98	-2.53	2.30	-2.48	-2.53	2.61	-1.62
1 and 4	5.29	4.83	5.42	7.56	6.82	8.51	7.56	6.41	8.66
2 and 3	-5.28	2.43	-7.31	-6.89	3.22	-10.39	-6.89	3.52	-11.09
2 and 4	2.46	5.57	0.09	3.19	7.74	0.59	3.19	7.33	-0.81
3 and 4	7.73	3.14	7.40	10.09	4.52	10.99	10.09	3.80	10.28

COMPUTER SOURCE—MPR: sty40501.do, sty40502.do, sty40507.do, sty40508.do, sty40513.do-sty40516.do.

*Impact is statistically significant at 0.1 level, two tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

APPENDIX E

AFRICAN AMERICAN AND LATINO TEST-SCORE DIFFERENCES

Daniel P. Mayer
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AFRICAN AMERICAN AND LATINO TEST-SCORE DIFFERENCES[29]

Our analysis of the impact of educational vouchers on the test scores of African American and Latino students shows that vouchers had a positive impact only on test scores of African Americans. Further exploratory analyses reveal, however, that the African American students in private schools are not outperforming the Latino private-school students or even the Latino public-school students. A comparison of the average achievement test scores for African American and Latino students suggests that, after three years, the average reading and mathematics test scores of the Latinos who were offered a scholarship, of the Latino control students, and of the African American students who were offered a scholarship were statistically similar while the average reading and mathematics test scores of the African Americans in the control group were lower than the scores of students in any of the three other groups (see Tables 20, 21, and 22). This pattern holds after statistically adjusting for baseline test scores, family income, education, and welfare status. Figure E-1 presents adjusted mean test scores for Latinos and African Americans from base year through year three.[30]

The pattern is similar for the, reading, mathematics, and combined test scores. For instance, after one year, the adjusted mean for an African American control is noticeably lower than the mean for a similar African American student who received a scholarship and a comparable Latino in the control group and for a Latino in the scholarship group. After three years, the adjusted means for the Latino voucher and control groups and for the African American scholarship group were similar. The adjusted mean for an African American control group student was 7.9 percentile points lower in reading, 8.3 points lower in mathematics, and 8.1

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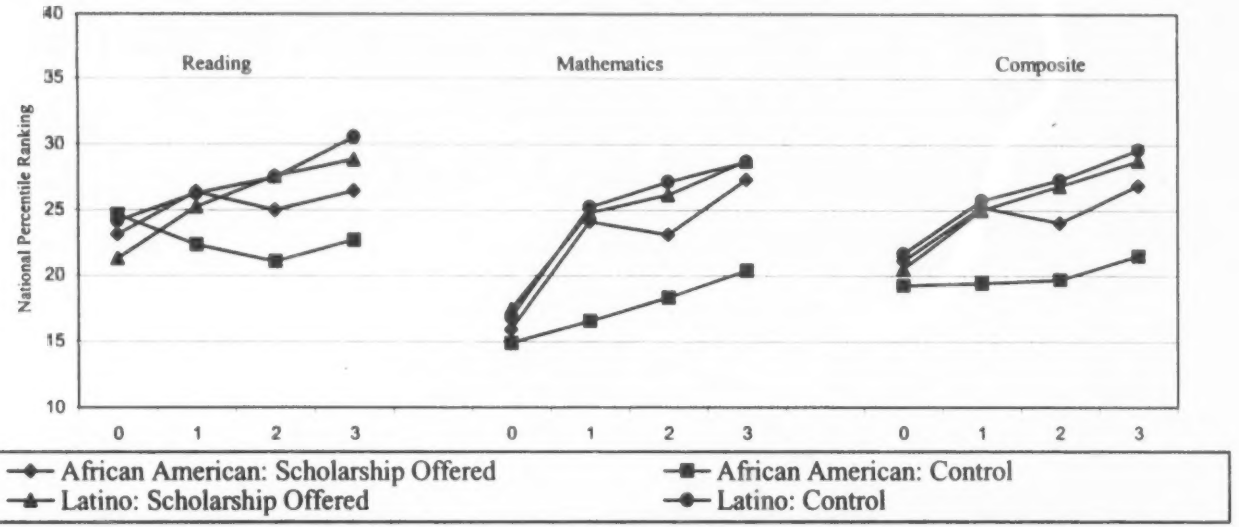
[29] Throughout this section, when necessary, we use regression analysis to impute values for school characteristic and family background to adjust for survey nonresponse, thereby maintaining identical samples as those presented in the previous section. Imputation was implemented at the family level separately by treatments and controls and by using race, baseline test scores, number of children in the family, lottery group, type of school at baseline, family income, and mother's education as predictors. For the indices, we included these background characteristics and nonmissing items from each index. For mother's country of birth, if there was missing information in the baseline but answers on one of the follow-up surveys, we used the value from the follow-up survey.

[30] The models used to adjust the means included the baseline test score (except for the baseline test-score model), mother's education level, family welfare status, family income, and the sample stratification variables. Adjusted means were calculated by setting these variables to the mean for the overall sample. These figures do not adjust for noncompliance (controls who went to private schools or treatments who did not use the scholarship), but the noncompliance rates of the two groups that took the examination in the third year are very similar: 86 percent of African Americans and 85 percent of Latino controls went to public schools and 68 percent of African Americans and 61 percent of Latinos ever switched to private schools, which suggests that test-score differences among groups were not a function of different compliance rates.

FIGURE E-1
ADJUSTED TEST SCORE MEANS FOR AFRICAN AMERICANS AND LATINOS



The adjusted means reported in these illustrations were obtained by developing a regression model for the test scores, controlling for treatment status, baseline test scores and the stratification group (number of children in the family, lottery group, and type of school), family income, receipt of welfare benefits, mother's education, and whether the mother was born in the United States. Model for the year 0 test scores did not include the baseline test scores as covariates. The "predict" command in STATA was then used to get the predicted numbers separately for treatments and controls, substituting the mean values (across the entire sample) for the other covariates.

Computer Source--MPR: fay40207b.do, fay40207c.

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performed significantly better in private schools than in public schools, Latino students performed as well as African American scholarship recipients regardless of which type of school they attended.

This finding suggests that if African American and Latino students attended different public schools, there might be important differences in these schools that would explain why the African American control students performed relatively worse than the three other groups. We created an index of dissimilarity (a measure used to compare how mixed or segregated two groups are) to ascertain whether African Americans and Latinos in the control group attended different public schools. The measure ranges from 0 to 1 and shows the proportion of one group that would have to move in order to achieve the same distribution as the other group.[31] We found that the students in our sample attended 420 public schools in the base year of the study and, by and large, that African American and Latino control-group students attended different schools. Seventy-seven percent of the African American students in our sample would have to move in order to achieve the same distribution among schools as the Latino students in the sample. This suggests that differences in the characteristics of public schools attended by African Americans and Latinos could explain why African Americans benefited from attending private schools, but Latinos did not.

To assess whether school characteristics differed for African Americans and Latinos, we used two data sources: administrative data from the New York City public schools and parent survey data collected by MPR. The administrative data, which are from the base year (before random assignment),[32] include 10 indicators of school quality: (1) total school enrollment, (2) percentage of teachers at the school who are fully licensed and permanently assigned, (3) percentage of students who did not stay enrolled for the entire year, (4) percentage of days students were absent, (5) percentage of students suspended, (6) percentage of students involved in disciplinary incidents, (7) percentage of students receiving free lunch, (8) percentage of students scoring at or above the state minimum the on the grade-three mathematics test, (9) percentage of students scoring at or above the state minimum on the grade-three reading test, and (10) pupil-teacher ratio.

A comparison of characteristics of schools attended by African American and Latino students in our sample shows that the schools they attended in the baseline year did *not* differ along 6 of these 10 dimensions: total school enrollment, percentage of teachers at school who are fully licensed and permanently assigned, percentage of students who did not stay enrolled for the

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[31] The index of dissimilarity is calculated in the following way: $.05 * \Sigma |P_{iL}/P_L - P_{iAA}/P_{AA}|$: where P_{iL} is the population of Latinos in our study in base-year school i, and P_L is the total population of Latinos in our study. P_{iAA} is the population of African Americans in our study in base-year school i, and P_{AA} is the total population of African Americans on our study.

[32] Because only 50 percent of the control group reported that its children were in the same school from the base year through the third year of the study, these findings are only suggestive of the types of schools the African American and Latino students in our sample attended throughout the three years.

entire year, percentage of students suspended, percentage of students involved in disciplinary incidents, and percentage of students scoring at or above the state minimum on the grade-three mathematics test (Table 28).[33]

The four dimensions on which the schools differed do not suggest that the schools attended by African Americans were worse than the schools attended by the Latinos. While absenteeism was higher in the schools attended by African American control-group students (11.5 percent of days versus 10.8 percent of days), the school poverty rates, measured by the percentage of students receiving a free lunch, and the pupil-teacher ratios were higher in the schools attended by Latinos in the control group (89.5 versus 86.6 percent receiving free lunch, and 17.2 versus 16.3 pupil-teacher ratio). The percentage of students scoring at or above the state minimum on the grade-three reading test was higher in the schools attended by Latino schools (64.5 versus 61.6 percent).

While the administrative data reveal no clear pattern of differences, other important characteristics of schools related to quality, such as class size (rather than the broader measure of pupil-teacher ratio),[34] the frequency of disruptions, the availability of various resources (such as a computer laboratory, library, or gymnasium), and the extent of communication between schools and parents, were not captured by these data. However, these characteristics are reflected in the data from our parent survey. Specifically, parents provided information on the following school characteristics:

- Disruptions: destruction of school property, tardiness, missing classes, fighting, cheating, racial conflict, carrying guns or other weapons, using drugs or alcohol
- School resources: computer laboratory, library, gymnasium, cafeteria, child counselors, nurse's office
- School programs: programs for non-English speakers, tutors for individual needs, programs for students with learning problems, programs for advanced learners, a music program, an arts program, an after-school program
- School communication with parents: parents informed about student grades halfway through the grading periods, parents notified when students are sent to the office the first time for disruptive behavior, parents are asked to speak to classes about their jobs, parents participate in instruction, parent open-house or back-to-school night held at school, regular parent-teacher conferences, parents receive notes from teachers

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[33] For these comparisons, school-level data were assigned to the students and then the difference in means was tested.

[34] Pupil-teacher ratios are an imprecise indicator of class size because they do not account for frequently intense teaching resources targeted toward Title I, special education, or bilingual services in some schools. When schools use those resources to attain low ratios for the targeted populations, they mask much higher ratios for the remaining classes in the school.

about student performance or behavior, parents receive a newsletter covering school activities/events

- School size
- Class size
- Racial segregation in the classrooms[35]

The parent survey data support the findings based on the administrative data (see Table E-1): along several dimensions, there were no differences in the public schools attended by African American and Latino students in our sample. Specific to the parent survey data, there were no differences in resources, class size, the frequency of school disruptions, the number and types of programs offered in the school, or the percentage of students attending completely segregated classrooms. The only significant difference was that, based on a comparison of parents' reports, schools attended by African American students communicated less with parents than did schools attended by Latino students.

Differences in school characteristics do not appear to explain the differences in test scores between the African American and Latino controls. And yet our examination of test-score trends over three years suggests that there was an interaction between the African American students and their public schools that may have affected them adversely when compared with the Latino students who remained in the public schools, the Latino students who attended private schools, and the African Americans who attended private schools. Knowing more about what makes the public-school experience for these two groups differ so much could provide important information regarding how to improve the public-school experience of African Americans. For the time being, however, the results presented here suggest that African Americans test scores

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[35] Indices were created for four categories of school characteristics. Disruption is a cumulative measure of whether the parent indicated that the following were a somewhat serious or very serious problem at his or her child's school, with a maximum value of eight: students destroying property; tardiness; students missing classes; fighting; cheating; racial conflict; guns or other weapons; drugs or alcohol. Resources are measured on a scale of 0 to 6, with a point given for each of the following facilities that are available to students: computer laboratory; library; gymnasium; cafeteria; child counselors; and nurse's office. Programs are similarly measured, on a scale of 0 to 7, with a point each for programs for non-English speakers, individual tutors, programs for students with learning problems, programs for advanced learners, a music program, an arts program, an after-school program. School communication is another cumulative measure; a point is given for whether the parent indicates that each of the following practices exists in his or her child's school (with a maximum of 8): parents are informed about student grades halfway through grading period; parents are notified when student sent to the office for the first time for disruptive behavior; parents speak to classes about their jobs; parents participate in instruction; a parent open-house or back-to-school night is held at school; parent-teacher conferences are held; parents receive notes about their student from the child's teachers; parents receive a newsletter about events in their child's school/classroom.

TABLE E-1

AFRICAN AMERICAN AND LATINO CONTROL-GROUP SCHOOL CHARACTERISTICS

	African American	Latinos	Difference in Characteristics
New York City Administrative Data			
Total school enrollment	9.00	9.47	-0.47
Percent of teachers at school who are fully licensed and permanently assigned	77.75	77.14	0.61
Percent of students who did not stay enrolled for the entire year	90.86	91.14	-0.29
Percent of days students were absent	11.51	10.64	0.87***
Percent of students suspended	1.99	2.06	-0.07
Percent of students involved in disciplinary incidents	1.18	1.00	0.18
Percent of students receiving free lunch	86.94	89.50	-2.56**
Percent of students scoring at or above the state minimum the on grade-three mathematics test	89.44	90.20	-0.75
Percent of students scoring at or above the state minimum on grade-three reading test	61.57	64.45	-2.88**
Pupil-teacher ratio	16.29	17.38	-1.09**
Parent Survey Data			
Disruptions	3.58	3.77	-0.19
School resources	5.52	5.43	0.09
School programs	5.43	5.19	0.23
School communication	5.29	5.99	-0.70***
Class size	27.03	27.35	-0.32
School size	487.50	509.38	-21.88
Racial segregation in the classrooms	0.43	0.39	0.04
(N)	491-545		

COMPUTER SOURCE—MPR: fay40107j.do, fay40107k.do.

*Impact is statistically significant at .10 level, two-tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

will increase if African American students attend private schools, but the Latinos do as well in both public and private schools.

Test-Score Impacts for First- and Later-Generation Latinos

Research suggests that first-generation Latino students perform significantly better in the schools than their counterparts from later generations.[36] Given that there are a substantial number of students from these two groups in our sample, we examined the impact on each group of the offer of a scholarship. In an exploratory analysis, we found no impact of the offer of a voucher based on the reading, mathematics, or combined test scores of later generations of Latino students, thereby suggesting that they perform similarly in public and private schools. For first-generation Latinos, although there is no impact on the combined test scores or on the mathematics scores, there is a *negative* impact on reading scores (see Table E-2).[37]

First-generation Latinos who were offered a voucher scored 3.4 percentile points lower after three years in reading than the control-group students (effect size = .15). After controlling for family income, education, welfare status, and base-year test scores, we observed that the negative effect of a voucher was still present. Figure E-2 presents adjusted means for first and later generations of Latinos from the base year through year three. The adjusted means for reading in the third year show that a first-generation student who was offered a scholarship scored 3.2 percentile points lower than a similar student who was not offered a scholarship. No differences are apparent when examining the combined test scores or the mathematics test scores.

Of particular interest with respect to the negative impact on the reading test scores of Latino students is the fact that first-generation Latinos who were offered a voucher were much less likely than the control group to receive English as a Second Language (ESL) services. While 79 percent of Latino control-group students attend schools with special programs for non-English speakers, only 51 percent of treatment-group students attend schools with such programs. Including ESL services as a covariate in a regression analysis that tests treatment- and control-group differences in reading does not, however, reduce or eliminate the negative impact.

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[36] A review of this literature is presented in Valenzuela (1999).

[37] We defined first- and later-generation status by sorting Latino students into two groups based on whether their mothers were or were not born in the United States (first-generation students), as reported in the parent survey.

TABLE E-2

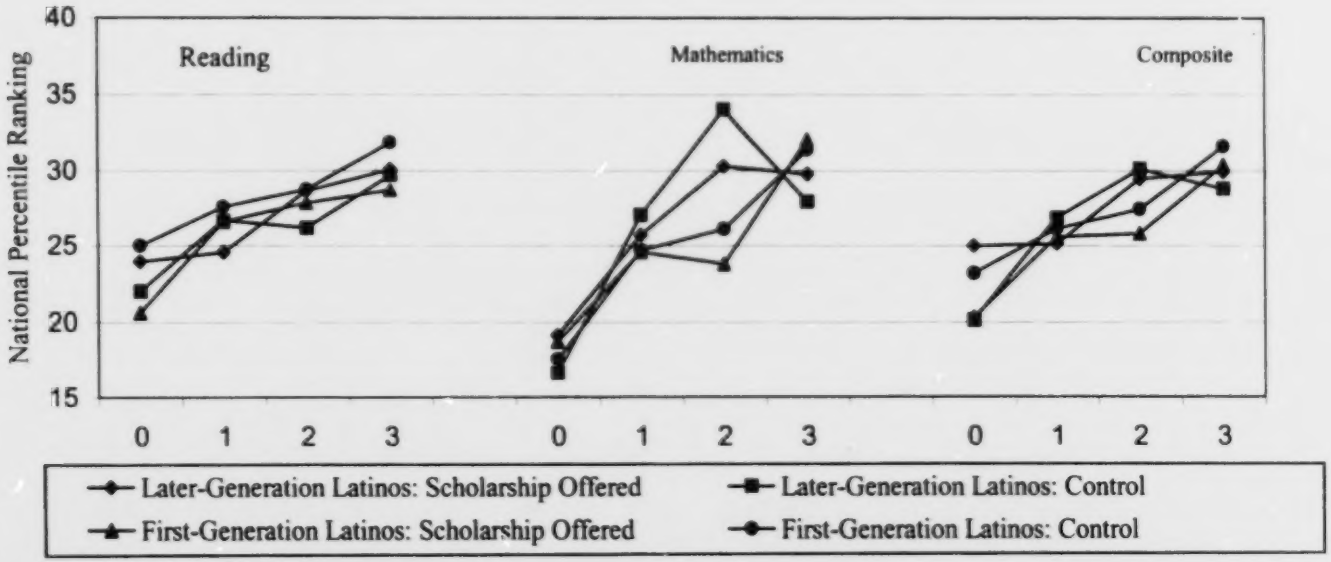
TEST SCORE IMPACTS FOR FIRST AND LATER-GENERATION LATINOS (YEAR THREE)
(Percentile)

	Reading			Math			Combined			
	Scholarship Offered (1)	Control Group (2)	Scholarship Offer Impact (3)	Scholarship User (4)	Control Group (5)	Scholarship Offer Impact (6)	Scholarship User (7)	Control Group (8)	Scholarship Offer Impact (9)	(N) (10)
First generation										
Overall	25.99	29.36	-3.37*	29.03	28.60	0.43	27.51	28.98	-1.47	455
Grades 4+5	23.57	27.72	-4.15	28.09	27.57	0.52	25.83	27.64	-1.81	237
Grades 6+7	28.46	30.12	-1.66	29.99	31.01	-1.02	29.22	30.56	-1.34	218
Grade 4	26.21	31.16	-4.95	31.49	27.68	3.81	28.85	29.42	-0.57	121
Grade 5	20.95	22.71	-1.76	24.72	24.82	-0.10	22.84	23.77	-0.93	116
Grade 6	28.81	32.54	-3.73	31.34	27.71	3.63	30.08	30.13	-0.05	124
Grade 7	27.97	25.10	2.87	28.13	32.63	-4.50	28.05	28.87	-0.82	94
Later generation										
Overall	28.86	29.52	-0.66	26.80	25.15	1.65	27.83	27.34	0.49	182
Grades 4+5	28.35	26.34	2.01	23.57	18.23	5.34	25.96	22.27	3.69	110
Grades 6+7	29.95	32.97	-3.02	33.68	31.18	2.50	31.81	32.07	-0.26	72
Grade 4	27.65	24.33	3.32	20.09	13.39	6.70	23.87	18.86	5.01	57
Grade 5	29.23	29.00	0.23	27.90	24.94	2.96	28.57	26.98	1.59	53
Grade 6	25.72	28.52	-2.80	28.32	23.50	4.82	27.02	26.01	1.01	46
Grade 7	37.13	38.48	-1.35	42.80	47.45	-4.65	39.97	42.97	-3.00	26

COMPUTER SOURCE—MPR: sty40219.do, sty40220.do.

*Impact is statistically significant at .10 level, two tailed test.
**Impact is statistically significant at .05 level, two-tailed test.
***Impact is statistically significant at .01 level, two-tailed test.

FIGURE E-2
ADJUSTED TEST SCORE MEANS FOR FIRST AND LATER GENERATION LATINOS



The adjusted means reported in these illustrations were obtained by developing a regression model for the test scores, controlling for treatment status, baseline test scores and the stratification group (number of children in the family, lottery group, and type of school), family income, receipt of welfare benefits, mother's education, and whether the mother was born in the United States. Model for the year 0 test scores did not include the baseline test scores as covariates. The "predict" command in STATA was then used to get the predicted numbers separately for treatments and controls, substituting the mean values (across the entire sample) for the other covariates.

Computer Source--MPR: fay40207b.do, fay40207c.

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Although we undertook this exploratory analysis when examining the difference in impacts for African American and Latino students, we want to emphasize that the negative impact on reading may be idiosyncratic and should be interpreted cautiously.

There are two reasons why it may be idiosyncratic. First, at baseline, the reading test scores of the first-generation control students were statistically significantly higher than those of the first-generation treatment students, thereby suggesting that the randomization process did not create two equivalent groups in terms of reading scores.[38] These differences were accounted for in the estimates presented for the follow-up years in Figure E-2 and in the negative impact reported in Table E-2, but there still may be non-observable characteristics contributing to the negative finding. Furthermore, a statistically significant impact is present only at the third follow-up and not at the first or second follow-up.

[38] Random assignment can break down when small samples of treatment- and control-group students are identified, and relatively large differences between the two groups for variables measured before randomization may be found.